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THE FEMALE SEX RHYTHM

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INTRODUCTION

Rhythm is a universal characteristic of natural phenomena. Wave formation of physical energy is well established. An analysis of periodic reactions produced by physico-chemical processes has been recently attempted by Hedges and Myers (66). They recognize "periodic structures," as well as the more familiar time periodicities. They point out the suggestive similarity between physical periodicities and the periodic functions of living organisms.

Of the biological rhythms, none is more striking than the female sex rhythm. So striking is this rhythm with its menstrual phenomena in women that it gripped the imagination of primitive peoples who lacked sufficient information to regard it as a natural event. Ellis, in a general discourse on the menstrual cycle (42), reviews the literature revealing the primitive attitude toward it, and gives a wealth of illustrative material showing the superstition and taboo with which it was surrounded. The menstruating woman was regarded with fear. She was required to refrain from all household duties, especially the preparation of food. Traces of this attitude may still be found in modern times. Scientific truth has shown it to be partly justified, since the work of Macht and Lubin (100) and others has revealed toxic substances in the secretions of menstruating women.

The most conspicuous aspect of the menstrual cycle is the hemorrhage, during which, according to Lahille (90), an average of from 50 to 55 grams of blood is lost. While individual differences occur in the frequency of recurrence of the bleeding, the most reliable statistics

include the majority of cases between 26 and 30 days. Studies using questionary methods, such as the classical work of Sanes (**128**) and its recent verification by Hajek (**59**), which depend on the subjects' unchecked recall, have reported much greater regularity in the menstrual function than the more carefully controlled records of such investigators as King (**86**) and Allen (**8**). Before we can properly evaluate studies on this topic more data are necessary. As Papanicolaou (**115**) suggests, "the need is a larger number of observations on individual cases of normal women over a long period of time."

The menstrual phenomenon is not a distinctly human one. Wiltshire (**160**) pointed out its universality in all races of men and also in the higher mammals. In a more recent study Hartman (**61**) has extended the scope of "menstruation" to include the vertebrate series beginning with the Salachian fishes. "Throughout the vertebrate series wherever the embryo lives in a brood chamber at the expense of the parent beyond the point to which the stored yolk of the egg carries it, there one finds bleeding into the brood chamber." The homology between the sexual cycles of the lower vertebrates and the human menstrual cycle is less clearly indicated than in the case of the mammalian sexual or oestrous rhythms. Papanicolaou (**111**), making a comparative study of animal sexual cycles, distinguishes between polyoestrous and monoestrous rhythms. He regards the former as peculiar to mammals, while the latter represent a seasonal fixation of sexual activities appearing in every class of animals. Cannon (**24**) regards the monoestrous rhythm as phylogenetically prior to the polyoestrous. He believes that as animals live under more sheltered conditions and the chances for sexual congress become correspondingly fewer, polyoestrus replaces monoestrus as affording greater certainty of procreation. He further distinguishes the oestrous cycle of lower mammals from the menstrual cycle of the primates. Although there has been much controversy as to the exact relationship between these two cycles, corresponding stages may be worked out, as we shall see below. Cannon raises the question as to whether or not there is any phase of the primate cycle that represents a true "heat" condition. Although this problem has not been finally settled, the period of sexual desire seems less definite and circumscribed than in the lower mammals. Wiesner (**159**) would further subdivide the polyoestrous group into diphasic and monophasic. The diphasic group shows sexual activity at definite stages followed by sexual antagonism, while in the monophasic group sexual activity is not limited. Animals with regularly recurrent "heat" periods,

such as rodents, would illustrate the diphasic group, while those which manifested no such pronounced phenomena, such as primates, would fall in the monophasic group. For primitive primates van Herwerden (71, 72) postulates a polyoestrous rhythm with a continual capacity for conception. The power of conception later becomes restricted to special periods, while the remaining menstrual periods occur without ovulation.

Phases of the Female Sex Cycle

While external signs of the female sexual cycle in animals have been noted by many observers, they have contributed very little to our knowledge of the different phases of the cycle. The reason why so little dependence can justifiably be placed in field observation is that the method is too crude to give a complete picture of the various phases and the transitions from one to another. The more exact morphological methods have revealed a reasonably regular sequence of histological changes in the mammals studied. These methods consist mainly of macroscopic and microscopic examination of samples of uterine, ovarian, or vaginal tissue, and of samples of the secretion found in the vaginal lumen. The operative methods, which involve extirpation of the tissue, often necessitate killing the animal. Moreover, human studies depend exclusively upon clinical material which is frequently atypical. The vaginal smear method, on the other hand, is applicable to living, normal subjects with practically no inconvenience. Since it has been found to be a delicate and reliable index of changes transpiring in other parts of the reproductive system, it has proved a valuable supplement to the other methods.

As early as 1847 Pouchet (119), using a crude technique, microscopically examined samples of the vaginal secretion of a variety of mammals. It was not until 1917, however, with the publication of Stockard and Papanicolaou's (141) pioneer work on the guinea pig, that the method was sufficiently refined to be used as a reliable index of oestrous phase. They introduced a small nasal speculum into the vagina and made a smear of any fluid present. A microscopic study of these fluids showed that the guinea pig has a sexual period lasting for about 24 hours and recurring " with striking regularity " every 15 or 16 days. According to the changes in the appearance and consistency of the vaginal fluid, four stages could be distinguished in the guinea pig's cycle. These results were confirmed by Selle (132), who used a modification of Stockard and Papanicolaou's technique. More recently Voss (151) has taken exception to the " striking regularity "

of the oestrous period reported by Stockard and Papanicolaou for the guinea pig, although his results have upheld the validity of the vaginal smear technique.

Since the work of Stockard and Papanicolaou, the technique they initiated has been widely adopted with various mammals. In 1922 Long and Evans (**94**) applied it to the albino rat and were able to work out homologous oestrous stages between the rat and the guinea pig in spite of the much shorter cycle in the rat. They included a tabulated review of previous literature on the mammalian oestrous cycle. Their results also appeared in a later publication by Evans (**45**). Ishii (**78**), summarizing work on the sexual cycle in the rat, criticized the vaginal smear method for failure to distinguish between a specific heat period and a more general oestrus. Very recently Hemmingsen (**67**), as a result of correlating vaginal smear criteria of heat in the rat with carefully graded behavior indications, has arrived at the conclusion that the relationship between intensity of heat and vaginal indices is not precise. The mouse was early studied by Allen (**1, 2**) with results very similar to those of Evans on the rat. Later work by Parkes (**116**), however, using a different selection of mice, revealed a cycle of 6.2 days as compared with the generally accepted 4.5-day cycle for the rat and mouse. These investigations soon ramified to mammals other than the rodents. A very primitive mammal of the marsupial order, the aplacental opossum, was studied by Hartman (**60**). He found that the cyclic changes in this low form were in every way comparable with those of the higher mammals. Many higher mammals have also been studied by this method. A comparative study of the sexual cycles of the rat, guinea pig, dog, cat, and man, by a combination of methods, has appeared in a recent monograph by Evans and Swezy (**46**), to which a comprehensive bibliography is appended.

The vaginal smear technique was first applied to primates in 1923 by Corner (**31**). His study of Macacus rhesus revealed some tendency toward rhythmical variation in the type of vaginal secretion, although the phases were not so clear-cut as in the rodents. More recently Corner (**33**) extended his study to Macacus irus, which he found to resemble closely the rhesus monkey. Hartman's (**62**) publication in 1932 of his extensive studies on Macacus rhesus verified Corner's findings on the cellular content of the vagina, although he employed a method of lavage analysis in preference to smear analysis. Using Hartman's methods, Ball (**13**) has recently measured physiological changes in the sex tract in eight female monkeys and cor-

related them with fluctuations in sexual activity. She found that although copulation was accepted by most of the females at any time during the month there was an increase in sexual activity just before ovulation and a decrease thereafter. She also noted wide individual differences not correlated with physiological changes in the sex tract. There was no evidence of a premenstrual rise. In 1932 Westman (**157**) reported morphological investigations on Macacus rhesus in which he noted a similarity between man and monkey in the processes of anatomical development and degeneration in the genital organs. His study includes a good review of the literature on the menstrual cycle in primates. Within the last few years several studies have appeared on the baboon, another member of the same family as Macacus. In this form attention is especially attracted to the striking changes in the sexual skin. Zuckerman and Parkes (**167**) regard the enormous swelling of the external genitalia as an oestrous phenomenon. This enlargement begins at the end of menstruation, reaches a climax at the time of ovulation, and thereafter subsides. In a later publication Zuckerman (**165**) compared the sexual skin activity in this form with the vaginal cornification in the mouse. Similar fluctuations in swelling have been reported by Woskressensky and Iwanow (**161**) in work with the same form.

Although a number of observational studies have been made on the anthropoid apes, very little exact work has been done. In 1931 Tinklepaugh and Campenhout (**144**) opened up this field in their study of a mature and an immature chimpanzee using a vaginal lavage technique. While they were able to distinguish the premenstrual from the postmenstrual phase in terms of the vaginal cell picture in the mature animal, the secretion of the immature ape failed to show the usual differentiation of various types of epithelial cells, and instead was characterized by small, vacuolized cells which suggested a nonfunctional condition. Recently Tinklepaugh (**142**) has published a continuation of his study of the older of the two chimpanzees, during four menstrual cycles and throughout pregnancy. Cyclic variations were observed in the vaginal cell content which were consistent with those found in monkeys. Moreover, as in the experiments on baboons, cyclic swellings of the sex skin were observed. Although a clearly defined period of sexual receptivity had not been found in monkeys, Tinklepaugh recognized a true oestrus in the chimpanzee "in common with most mammals below the primates, and with the baboon." He found the total length of the cycle to correspond closely with that of other primates including the human.

Much interesting material on the reproductive processes of mammals with special emphasis on the primate, together with an extensive bibliography, may be found in Zuckerman's (**166**) recent book on the social life of monkeys and apes.

In studies of the human, the vaginal smear technique has been used in pathology for a long time, but the application of this method to morphological and physiological studies is very recent. In 1924 Papanicolaou (**112**) made a preliminary report of results obtained by this method. He found that the normal human smears differ considerably in cellular composition from those of lower mammals, yet they show typical changes which may serve to indicate a number of distinct stages. Allen (**3**) also reported a decided variation in the cellular content at different times in the cycle, which was, however, less clear-cut than in rodents. The first attempt to apply the technique outside of the clinic was that of King (**85**), who obtained vaginal smears from seven normal women engaged in academic work, covering from two to three cycles each. She found that "the cellular content of the vaginal secretion of the normal human female is exceedingly variable and is a doubtful index of changes transpiring in the ovary and uterus." More recently Moser (**106**) has also reported negative results. She obtained vaginal smears from ten healthy women either daily or at two-day intervals over a period of one to two menstrual cycles. She was unable to distinguish definite phases of the cycle by this method. In the year of Moser's publication came the report of a study by Ramirez (**121**), in which scrapings from the vaginal walls of 37 women were studied. A quantitative cytological analysis of his material throughout the cycle revealed that various types of cell prevailed at various phases of the cycle, which gave support to the diagnostic value of the method. The discrepancies among the results of these investigators may be due to differences in the techniques used in the staining and preserving of the specimens. With improved methods, Papanicolaou (**114**) has succeeded in distinguishing various phases of the human menstrual cycle and has compared them with homologous phases in the oestrous cycles of lower mammals. In a new study (**115**) the same investigator presented further positive results. His data included daily vaginal smears from 12 healthy young women through two or three consecutive menstrual cycles. In addition he reported smear analyses of a normal woman covering 15 cycles. These results confirmed Papanicolaou's classification of the human menstrual cycle into four phases and one stage; the congestive (menstrual) phase, high follicular (postmen-

strual) phase, ovulative stage, proliferative (intermenstrual) phase, and secretory (premenstrual) phase.

On the basis of the studies just reviewed, it is possible to point out certain general features of the female sex cycle that hold with a fair degree of uniformity throughout the mammalian class. Four well-demarcated phases may be distinguished from one another. During the first, or prooestrus, congestive phenomena occur. The ovary shows growth and enlargement of the follicles, while the corpora lutea of the preceding cycle are undergoing fatty degeneration. The cell content of the vaginal secretion is primarily composed of epithelial cells. With the onset of oestrus the follicles continue to progress to the point of rupture. There is a discharge from the vagina which is histologically characterized by cornified cells and a scarcity of leucocytes. Oestrus ordinarily culminates in ovulation, which in the majority of mammals occurs spontaneously, although even in these forms it may be accelerated by coitus. Following ovulation the metoestrous phase of the cycle begins, during which new corpora lutea are developing from the cells of the ruptured follicle wall. The vagina shows a massive desquamation of epithelial cells together with a sudden infiltration of leucocytes which dissolve the epithelial cells. During the last phase, dioestrus, the corpora lutea are mature and in full function. The corresponding stage in the vagina is marked by a smear of irregular character with atypical epithelial cells and numerous leucocytes. If the ovum is fertilized pregnancy ensues, with persistence of the corpora lutea and resultant suppression of succeeding ovulations. If fertilization fails to occur the corpora lutea degenerate and prooestrous changes begin.

These ovarian and vaginal changes are paralleled by changes in the uterine mucosa, but the differences among mammalian forms are so great as to make generalizations practically impossible. Contributory mechanisms in the oviducts and mammary glands also show cyclic variations simultaneous with those in the uterus, ovary, and vagina.

Since perfect correlation between the various stages of the oestrous cycle in the lower mammals and the menstrual cycle in primates has not been established, there are differences in interpretation of menstruation. By some it is regarded as representing degenerative changes at the end of the cycle, by others as homologous with the prooestrous bleeding which ushers in a new cycle in certain lower mammals, while several have suggested compromises or "telescope theories" according to which menstruation is a combination of degen-

erative changes of one cycle with prooestrous changes of another. In general the postmenstrual phase corresponds with the oestrus of lower mammals, intermenstrual with metoestrus, and premenstrual with dioestrus. A review of the comparative physiology of reproduction has just been published by Hartman (**63**).

Physiological Mechanism of the Female Sex Rhythm

Many speculations have been hazarded as to the causation and significance of these morphological changes. One of the earliest explanations of menstruation was advanced in 1865 by Pflüger (**117**). According to him, menstruation is the result of a pelvic hyperemia induced reflexly by the pressure of a growing Graafian follicle on the ovarian nerve terminations. This was later disproved by the experimental work of Knauer (**88**) involving removal of the ovaries with consequent severing of their nervous connections. Under these conditions menstruation did not cease provided the ovaries were transplanted elsewhere in the body. These results led Knauer to the belief that the ovarian influence is exerted through the blood stream rather than the nervous system. At that time, however, little was known about the details of the mechanism. Since those days great advances have been made in the analysis of the chemical factors concerned in the mammalian female sex cycle.

Early investigators stressed the hormones of the corpus luteum. Beard (**14**) compared menstruation to an abortion, an abortion of a decidua prepared for an ovum which had escaped fertilization. He (**15**) suggested that the decidua is prepared through the function of the corpus luteum, and that degeneration of the corpus luteum at the end of the menstrual cycle results in its abortion, just as degeneration of the corpus luteum before the end of gestation results in parturition. This abortion makes possible the resumption of ovulation, which has been held in check during the normal functioning of the corpus luteum. A serious criticism of the abortion theory was its failure to explain cases observed in primates by many investigators, in which menstruation occurs without previous ovulation. Since the corpus luteum does not develop in such cases, it cannot cause the destructive phenomena. A wealth of experiments, of which we can cite only a few examples within the limits of this review, has shown that the sexual cycle is conditioned by an exceedingly complex interaction of hormones.

The ovarian follicle was found by Allen (**7**) and Frank (**52**) and their collaborators to secrete a hormone, the "female sex hormone,"

which stimulates activity in the genital tract and produces oestrus and ovulation. According to Allen (**4, 5, 6**) it is the sudden withdrawal of this hormone from the blood stream that causes the menstrual bleeding in primates. The corpus luteum, which develops after ovulation from the cells of the old follicle walls, continues the secretion of female sex hormone and, in addition, produces a specific hormone which has been found by Papanicolaou (**113**) and others to inhibit oestrus and ovulation. At the same time, according to the studies of Corner and Allen (**10, 35**), the luteal hormone has the important positive effect of continuing the genital growth activity initiated by the follicle hormone, until the uterine mucosa has attained a sufficiently proliferated condition to allow the implantation of fertilized ova and the maintenance of pregnancy. The female sex hormone and the luteal hormones bear important temporal relationships to each other. Allen (**9**) and Hisaw and Leonard (**73**) have shown that the female sex hormone is a prerequisite to the proper functioning of the corpus luteum. According to them it puts the uterus into a physiological condition for response to the luteal hormone.

The endocrine basis of the sexual cycle does not end with the ovarian hormones. Evidence has been accumulating which shows that without the action of certain pituitary hormones no ovarian function can take place. In 1926 Zondek and Ascheim (**163, 164**) and in the same year Smith (**136**) showed that the anterior pituitary was necessary to activate the ovaries. As a result of further experimenting, Zondek (**162**) has postulated two anterior pituitary sex hormones, one stimulating follicular growth and the other, luteinization. Smith and White (**137**), and others, however, have left open the question of the duality or unity of the ovary-stimulating pituitary hormone. Recently Moore and Price (**102**), Spencer, D'Amour, and Gustavson (**139**), Saiki (**126**), and others have shown that pituitary activity is in turn controlled to some extent by ovarian hormones, which if present in the necessary amounts reduce it. When this occurs there follows further decrease in female sex hormone, finally resulting in bleeding. These reciprocal relations between pituitary and ovarian secretions may be the source of the rhythmic nature of the sexual cycle. Another interpretation, however, suggests that the rhythm may be confined to the ovary, which alternates between a susceptible and a refractory stage to the action of the pituitary.

This necessarily sketchy treatment of a vast field of research may indicate that although great progress has already been made in

analyzing the endocrine basis of the female sex rhythm, the final picture awaits the results of future investigations. Excellent summaries of the developments in this field may be found in English by Corner (**32, 34**) and Hartman and Novak (**64**), and in German by Clauberg (**26**).

Effects of the Female Sex Rhythm

Cardio-Vascular Changes

In the analysis of the physiological factors underlying the female sex cycle, the problem was to determine how various factors affected the cycle. A further problem is to determine how the female sex cycle affects other functions. Early speculation suggested a "wave theory of menstruation," according to which the biological functions of women show a cyclical ebb and flow determined by the sexual rhythm. Goodman (**54**), one of the earliest advocates of this theory, regarded the menstrual rhythm as an index of a general condition of the vascular system which is locally manifested in the generative organs. In view of this linking of the menstrual wave with vascular changes, it is not surprising that the earliest attack on the problem was through studies primarily concerned with changes in pulse and blood pressure accompanying the menstrual cycle. Eagleson (**40**) presents a good review of this literature up to 1923. From 1870 to 1910 the blood pressure studies seemed to give evidence in support of the wave theory. They were practically unanimous in reporting a premenstrual rise which reached its maximum either just preceding the menstrual flow or on the first day, followed by a drop which reached its minimum at about the middle of the hemorrhage phase and gradually rose to normal during the postmenstrual period. The pulse curves were not so clear-cut but on the whole fell in line with those obtained on blood pressure. Among the earlier investigators, von Ott (**110**) has been closely identified with the wave theory as a result of his experiments on a large number of biological functions in about 60 healthy women. A summary of his results revealed a typical sine curve with a premenstrual rise and menstrual fall. Reviewing the work of von Ott and that of other early investigators, Fränkel (**50**) regards them as having established "in den Gesamtlebensprozessen des Weibes ein Auf und Nieder der wellenartigen Kurve mit einer Acme kurz vor und einem Wellental kurz nach der Menstruation." In spite of the fact that these results all point in the same direction, we cannot agree with Fränkel in his uncritical acceptance of them, because of obvious defects in technique. In most studies the results

were based on insufficient data and no attempt was made to determine the statistical significance of the changes reported, so that it is impossible to tell whether they represent true differences or are merely the product of chance variables. An even more serious criticism is the omission of control groups. Mosher (107) was the only early investigator to introduce a group of men to check against the women, and she found a rhythmical fall at definite intervals in the results obtained on the men as well as in those of the women. The fall in the women's blood pressure, to be sure, coincided with menstruation. Although her data indicate a blood pressure rhythm that may indeed be a sexual one, it cannot be regarded as an expression of an exclusively *female* sex rhythm. To what extent the same thing is true of the other studies we cannot tell.

During the decade between 1910 and 1920, a number of studies employing improved experimental techniques reported negative findings on blood pressure changes. After this the experimental literature rapidly increased. In 1923 Moore and Cooper (104) studied menstrual variations in blood pressure, pulse rate, and respiratory rate. They took records on eight subjects from four to six days each week throughout an academic year. The trials were always made at the same time of day for each subject and after a preliminary rest period. In treating their data they made composite curves covering several cycles in order to cancel out the chance variations which tended to obscure the periodic rhythms. After these composite curves had been smoothed, they showed a low menstrual phase followed by a slight, very brief postmenstrual rise. The highest sustained part of the curves occurred between the 14th and 18th days. There was a characteristic drop just before a premenstrual rise which failed to reach the intermenstrual level. The low menstrual and high intermenstrual periodicity was especially marked and uniform in the pulse rate, respiration rate, and circulatory index which consisted of pulse pressure times pulse rate. The writers, however, point out the fact that since the "menstrual" variations are not greater than many chance variations, they do not signify any extensive general lowering of physical condition. A Japanese investigator, Ishikawa (79), reported having found an increase in blood pressure at the beginning of menstruation followed by a decrease during the period. After the period there was an increase up to normal or above. During the intermenstrual period there were no marked changes. He also found vital capacity and other vital functions to vary in the same way as blood pressure. Since we must depend on

secondary sources for an account of this material, it is impossible to criticize it.

One of the most thorough experiments on periodic variations in circulatory functions is that of Truesdell and Croxford (**145**) in 1926. They used the "circulatory efficiency test" for aviators to give an estimate of the general physical condition of the six women who served as subjects. This involved pulse and blood pressure determinations before and after exercise. The results, covering from six to eleven cycles for each subject, showed a higher rating in circulatory efficiency during the menstrual period and a slightly lower rating between periods. The systolic blood pressure showed a rise previous to and in the early part of the period, while the diastolic pressure showed one rise during the period and another midway between. The authors concluded that rhythmic variations in both blood pressure and pulse are discernible. More recently Scott and Tuttle (**131**) also used an index of circulatory efficiency in the form of the ratio of the resting pulse rate to the pulse rate following standard exercise. Tests were given to 78 women between the ages of 16 and 41, three days preceding, during, and on the third and twelfth days after the cessation of the flow. When the differences among the four phases were statistically analyzed it was found that there was no difference as great as four times its P.E. The authors concluded from this that the differences observed must have been due to causes other than menstruation. A little later Eichbaum (**41**), working with healthy women, obtained inconclusive results on blood pressure. The expected rhythm of a premenstrual rise and a menstrual fall appeared in only half the cases. A more recent study by Grollman (**56**) on the effect of the menstrual cycle on the cardiac output, pulse rate, blood pressure, and oxygen consumption of a normal woman has also failed to show significant variations due to menstruation. Moore and Jenkins (**105**), on the other hand, reported positive results. They took daily pulse rates of seven women over periods of from two to thirteen months, and found variations with each phase of the menstrual cycle. There was a progressive increase from the end of the first to the end of the third week, followed by a progressive decrease during the remaining two weeks. The lowest point appeared late in the menstrual period or after the cessation of menstruation. They found the differences between the high and low values in the rhythm to be statistically reliable.

Summarizing the studies just reviewed, we shall have to conclude that research has failed to reveal consistent blood pressure variations

accompanying the female sex cycle, and that the wave theory of menstruation, with respect to circulatory changes, has not been supported by recent evidence.

Body Temperature

Another approach to the question was through the study of cyclic changes in body temperature. For a general review of the studies before 1924 the reader is again referred to Eagleson (**40**). The early investigators, under the immediate influence of the wave theory, with few exceptions agreed on a premenstrual rise followed by a menstrual fall. The classic study was done by van de Velde (**147**) in 1904. On the basis of a study of about a dozen women between 25 and 50 years of age, he recognized a cycle in the temperature curve which he attributed to ovulation. Ovulation occurs when the curve is at its lowest point, after which it rises regularly to a peak and then drops to the next low by the time of the next ovulation. The menstrual flow begins during the phase of lowering temperature prior to ovulation. In a recent publication he (**148**) has elaborated his theory. He asserts that the sudden rise in temperature is caused by the functioning of the corpus luteum following ovulation, and considers the zenith as the date of the final disintegration of the ovum. While this is an interesting speculation, it must remain merely a speculation until experimental research proves whether there is causal relationship or merely concomitance between the functioning of the corpus luteum and cyclic temperature rises. Meanwhile there appeared the work of Flaskamp (**49**), who, on the basis of exact temperature measures on healthy women, reported a premenstrual rise in temperature, with a second rise at about the 14th day of the cycle. The author brought these changes into relationship with the metabolic processes accompanying the menstrual cycle, correlating temperature rises with rises in the general functional level preceding menstruation and ovulation; temperature drops, with subsequent catabolic phases. The preovulatory rise is not consistent with van de Velde's findings. It is difficult to evaluate this report, since it is merely the abstract of a paper and the specific methods and results are not presented. Flaskamp's reported preovulatory rise did not appear in the more recent data of Jacoby (**80**) on 21 healthy women. While some subjects' curves revealed no rhythmic fluctuation, 17 showed a significant rise from seven to nine days before menstruation and a drop of from .5° to .8° C. with the onset of the hemorrhage or from one to two days

preceding. The temperature reached a minimum toward the end of the bleeding. There was no secondary rise at the time of ovulation. A very recent study was reported by Harvey and Crockett (**65**). The main part of their study was concerned with oral clinical readings of temperature of a woman, with her husband as a control subject. The readings were made daily at 7 A.M. for 106 consecutive days. Their results show a consistent regularity in the curves for the woman. The menstrual flow commences while the temperature is dropping. After reaching its lowest point a few days after the cessation of the flow, the curve rises to a peak and once more drops to a low through the menstrual period. The husband's curves do not exhibit the significantly consistent wave-like fluctuations of his wife's. This study is interesting from a methodological standpoint, since the experimenters introduced refinements in the statistical treatment of their data that had not been used in previous studies. They constructed a composite curve for all the cycles studied and then determined an equation of best fit to the coördinates of the mean temperatures in the composite cycle. They point out the advantage of this method in revealing individual differences in temperature changes.

The results of the studies on body temperature which we have reviewed seem in general to support the wave theory. They show a fairly consistent tendency toward a premenstrual rise followed by a menstrual fall. Pathological cases, particularly tuberculosis and endocrine disturbances, which lie beyond the limits of this review, display even more striking premenstrual rises, because, as Bezançon (**19**) has pointed out, such patients show sympathicotonic tendencies. Within the normal range of variation, tendencies toward sympathetic reaction may account for the hyperthermic trends noted.

Metabolic Rate

Closely related to body temperature changes are changes in heat production, which are measured by the amount of carbon dioxide exhaled or oxygen consumed in a given period of time. Basal metabolic rate is an index of thyroid activity, which, possibly because of a phylogenetic origin from the uterus, bears a synergic relation to gonad function and consequently might be expected to show cyclic variations. Some modern evidence upon thyroid-ovarian relationship was supplied by Labhardt and Hüssy (**89**) in 1922. They found a swelling one to two days before menstruation, reaching its maximum on the first day and then decreasing.

The correlated changes in basal metabolic rate have been studied by many recent investigators. There was very little development along this line before 1920, but from that time on the studies have multiplied very rapidly. Boothby and Sandiford (**21**) have reviewed the literature up to 1924 and remark upon the inconsistency of the results. Where cyclic changes were found they were in the nature of a premenstrual rise followed by a menstrual fall. The reviewers, however, were unable to find constant variations with the menstrual cycle in their own work and suggest that the increase reported by others may be the result of pain and discomfort rather than of ovarian processes directly. Just before their review appeared, Wakeham (**152**) reported research involving 98 determinations of basal metabolism on 24 healthy subjects under very uniform conditions. He found clear-cut cyclic variations. There is conspicuous in his curve a premenstrual rise beginning less than a week before menstruation, followed by a decrease below normal at the beginning of or during the menstrual period. During the next seven to ten days there is a gradual return to the normal level. In 1924 Hafkesbring and Collett (**58**) criticized previous studies on the ground that the experimental periods were too short and at irregular intervals. They performed experiments on two subjects over a period of four months and found a premenstrual rise followed by a sharp drop on the first or second day of menstruation unless interfered with by pain. Collett and Liljestrand (**27**) also reported positive results on menstrual variations of basal metabolic rate. They found a double cycle in oxygen consumption, one peak occurring from four to ten days, the other from 15 to 19 days after the beginning of menstruation. In other words, in addition to the familiar premenstrual rise and menstrual fall, there was a postmenstrual rise and intermenstrual fall. These curves were based on only one woman's results compared with those of one man as a control. Observations were made over a period of three months, at intervals of one to four days. Stephani (**140**) confirmed the previous findings of a menstrual fall in metabolic rate. The menstrual lowering in oxygen consumption amounted to about 14 per cent. Lanz (**91**) used 11 subjects, the majority of whom were suffering from pleuritis while the others were healthy. From four to five determinations were made during a cycle. The results showed more clear-cut cyclic variations in the sick subjects than in the normals. In the former there was a rise which persisted through the first day of menstruation, after which there was a drop which reached its lowest point in the postmenstrual and early intermenstrual period.

While the healthy subjects also showed fluctuations, they were indefinite. This result is in line with the differences noted above between normal and pathological subjects in body temperature and is probably also referable to differences in sympathicotonic susceptibility.

Benedict and Finn (**18**) presented data on one healthy woman whom they tested intermittently for a period of 12 years. Comparing the deviation of the measured from the predicted heat production for 32 menstrual versus 89 intermenstrual days, they found a greater discrepancy in the direction of lowered metabolism on the menstrual days. A second series of measurements made on consecutive days through three menstrual cycles showed oxygen consumption to be lowest and most uniform during menstruation and highest about one week after its cessation. It should, however, be pointed out that their curves are very irregular and the tendencies they suggest, especially with regard to the peaks, do not seem very clear-cut. A few years later, Sandiford, Wheeler, and Boothby (**127**) continued observations on the subject used in Benedict and Finn's study. They obtained normal data covering 14 menstrual periods. In ten of these they found a slightly lower heat production during the flow, preceded by a premenstrual rise. The average difference between these two stages of the cycle was 0.8 ± 0.4 calories. Although this difference is not completely reliable, the chances are nine to one that it represents a true difference. They again suggested the possibility which they had earlier pointed out, that the results may have been affected by the subject's physical discomfort or mental status and consequently may not directly represent variations in the intensity of the oxidation processes. Gustafson and Benedict (**57**), in a study of seasonal variation in basal metabolism, incidentally observed menstrual variations in 14 of their 20 subjects. Although there is a hint that the metabolism is lowered during menstruation, the data are so meager and irregular as to preclude any final conclusions.

Another contribution to the study of menstrual cycle variations in basal metabolism was made by Hitchcock and Wardwell (**75**). They reported the results of 625 tests of 20 women subjects, covering at least two complete cycles each; 14 of these showed a decrease in rate during the menstrual period, five a slight rise, and one no change. In ten cases where the menstrual fall was statistically reliable it amounted to 5.16 per cent. The experimenters also noted an intermenstrual drop averaging 5.03 per cent occurring between the 12th and 23rd days, which suggested to them a relationship to ovulation. Griffith, Pucher, Brownell, Klein, and

Carmer (**55**), making long-time studies of three women and two men, found that menstruation had a decided effect on general metabolism. In agreement with the majority of previous investigators, they found the lowest rate during menstruation and the highest during the premenstrual week. The average difference of 2.7 per cent, however, was too small to be reliable. Confirmation of the general trend of these curves appeared in the less extensive studies of Rogers and Flemming (**124**) and Matters (**101**). Dellepiane (**36**), working with healthy women as well as with those suffering from various types of ovarian insufficiency, reported marked variations in basal metabolism even among the healthy, although of less magnitude than in the sick subjects. The rise began in the late premenstrual period and persisted into the early menstrual phase. The lowest points appeared early in the premenstrual phase as well as postmenstrually. DuBois' (**37**) 1930 review of work on basal metabolism included the chief recent studies on menstrual cycle effects.

In the studies so far reviewed there seems to be general agreement among investigators in spite of widely different techniques. They find cyclic variations in basal metabolism indicating a premenstrual rise followed by a menstrual fall. A few instances have also been reported of regular secondary intermenstrual fluctuations. Some of the more recent experimenters have reported less clear-cut results. Bonorino Udaondo, Raffo, and Biot (**20**), in a study on three healthy women, admit the probability of periodic metabolic variations, but in their opinion neither their own results nor those of previous investigators justify dogmatic conclusions because of the uncontrolled variables that may have affected them. Wible (**158**) is also cautious in interpreting the results of two years of experimenting with 22 subjects. This investigator agrees with those who report low oxygen consumption during menstruation, but states that there is no evidence of a premenstrual rise. The period of highest oxygen consumption is located two weeks after the cessation of menstruation. It should be pointed out that this is practically within the premenstrual phase and is consequently not at variance with the typical rhythm noted in previous investigations. Grollman (**56**), however, reports definitely negative results, not only for basal metabolic rate, but for pulse rate, blood pressure, and cardiac output. According to him, none of these physiological functions showed significant variations attributable to the menstrual cycle. The atypicality of these results may in part be due to personal idiosyncrasies of the "healthy woman" who acted

as subject. While these studies have been concerned with *basal* metabolism, Smith and Doolittle (135), working on *standing* metabolism, failed to find significant menstrual cycle fluctuations. With the exception of these few studies, the experimental literature has quite consistently revealed rhythmical patterns of change in metabolic rate.

Some experimenters have tried to correlate these changes with ovarian influence. Such an interpretation may be found in the work of Lee on basal metabolism in the albino rat. In one study (92) the basal metabolic rate was determined at different stages during the oestrous cycle for eight female albino rats from four to seven months of age. The heat produced per square meter of body surface per day showed consistent variations from the general mean only during the last ten hours of dioestrus and at the beginning of prooestrus. At that time an average increase of 12 per cent of the general mean was noted, which was statistically reliable. Even during oestrus, with its marked increase in excitability and voluntary activity, the average heat produced was below the general mean. In a later study the same investigator (93) found essentially the same results as he had in his previous work. These results are somewhat homologous with those for the human where a premenstrual rise followed by a menstrual fall were reported. In his critique of earlier studies, Lee points out the failure to make a sufficiently large number of determinations on a sufficiently large number of subjects during all stages of the cycle to make possible statistical treatment of the data. He also suggests the possibility that in cases where negative results were reported menstruation might have occurred without ovulation. If the premenstrual rise in metabolic rate is due to stimulation of the thyroid by the corpus luteum, the absence of ovulation would account for failure of the rise to appear.

A series of studies by Conklin, McClendon, and collaborators have suggested possible relationships between cyclic changes in basal metabolic rate and ovarian secretions in the human. McClendon, Conklin, Wildebush, and Wiles (97), after analysis of 438 almost daily basal metabolism determinations covering one or more cycles in ten women free from dysmenorrhea, found a premenstrual rise followed by a menstrual fall which reached its lowest level during the postmenstrual period. They point out that the secretion of the "ovarian hormone" follows the same curve. Conklin and McClendon (30) made a statistical analysis of these data. They divided the cycle into four periods and found the P.E.av. for each. They found the premenstrual rise to exceed the P.E. in eight of ten cases. The post-

menstrual fall below the intermenstrual norm was greater than the menstrual, but not quite so marked as the premenstrual rise. In one subject the findings were reversed. McClendon, Myrick, Conklin, and Wilson (**98**) later reported another study in which 342 determinations were made on 23 women. They attributed the slight rise which occurred in the period between menstruations to ovarian hormones.

Two publications during the past year have also suggested a correlation of the cyclic changes in metabolic rate with endocrine changes connected with the sex rhythm. Russo (**125**), working with 16 women, found a sharp rise in the premenstrual period followed by a drop which sank to its lowest value during the bleeding or immediately after it. He suggests that this suppression may be influenced by the ova or the corpus luteum. On the basis of results on ten normal women, Árvay and Meyer (**11**) assert a premenstrual rise and menstrual fall. They attribute these variations to variations in the ovarian hormonal content (luteal?) of the blood. Both of these articles include good reviews of the literature in their native languages.

A more direct attack on the problem of the relationship between ovarian hormones and cyclic variations in metabolic rate would take us too far afield and involve us in the extensive literature on injection experiments. Wang and Hawks (**153**) give a summary of this literature up to 1929.

While we have confined our attention in this survey to the work on menstrual cycle changes in *general* metabolism, it should be noted that an extensive literature on blood chemistry has accumulated in which cyclic effects on various *specific* kinds of metabolism are reported. This literature has been reviewed in German up to 1929 by Eufinger (**43**) and up to 1933 by Mühlbock (**108**).

Sensory Functions

So far we have been studying the effects of the menstrual cycle on the circulatory, respiratory, and metabolic processes. We are about to study functions more specifically involving the nervous system. On the sensory side, one of the earliest studies was made in 1887 by Finkelstein (**48**). He studied the functional activity of the eye during menstruation in 20 women. He found a concentric narrowing of the field of vision beginning one, two, or three days before, reaching the greatest intensity on the third and fourth days of the flow, and gradually disappearing on the seventh or eighth day after its appearance. Furthermore, he found that the fields of vision

for white and also for green, red, yellow, and blue underwent a regular diminution. An important contribution along this line has recently come from Lorenzetti (95). Testing the extent of the color field in artificial illumination with a perimeter, he found a slight but significant narrowing of the fields for red and green in the premenstrual period. During menstruation there was a gradual widening of the fields which reached the normal level in the postmenstrual period. In acuity of color perception measured in terms of the greatest distance from which a color was still recognizable, he found increased difficulty in the perception of yellow and blue in the premenstrual period, which disappeared completely during menstruation. Central color vision as measured by the Holmgren worsted test was unaffected by the menstrual cycle. In another study the same investigator (96) reported a case of visual disturbance occurring during menstruation and pregnancy, which prevented the subject from sewing, reading, or doing other detailed work. Lorenzetti attributes these disturbances to hormonal toxic influences. To what extent similar effects may occur in other sensory fields, future research must determine.

A recent study on cutaneous sensitivity was presented by Herren (68). He measured the tactual sensory threshold and the two-point thresholds for pain and touch. The tests were made within five days prior to the onset of the menses, within three days following the cessation of the menses, and on a day two weeks following the onset of the last menses. Five women between 24 and 34 years of age served as subjects in the experiment, and 11 menstrual cycles were tested in all. His premenstrual readings showed lower thresholds than those obtained in the postmenstrual phase. These differences in absolute magnitudes were .08 mgm. for the tactual threshold, 5 mm. for the two-point touch, and 6.9 mm. for the two-point pain threshold. Since the reliabilities of these differences were not given, we cannot tell how significant they are. The author attributed his findings to variations in the female sex hormone concentration in the blood, suggesting that the hormone increases the sensitivity of the cerebrospinal neurones to incoming impulses.

Reflex Action

On the motor side, there are a number of experiments on the effect of the female sex rhythm on reflex action. King (84) studied variations in the knee jerk daily over a period of from one to four menstrual cycles in nine women engaged in academic work. Her

records show a period of hyperexcitability in connection with menstruation but bearing no *constant* relationship to it. When this occurs it is followed by a fall and an approach to an intermenstrual average. Another experiment on the knee jerk is reported by Tuttle (146). In the five women who served as subjects no regular and uniform changes were apparent. In confirmation of King's results, however, Tuttle found that any disturbing influences affecting the knee jerk had less influence during the intermenstrual period than in the periods nearer menstruation. Very recently Herren and Haterius (69) have contributed to this problem in their analysis of the causal factors involved in oestrous variations in the Achilles reflex of the albino rat. In the first set of experiments, covering a period of 30 oestrous cycles on 16 female rats, they found a prolonged Achilles reflex time during oestrus and a correspondingly short time during dioestrus. They attributed these differences to an increase in the concentration of the follicular and luteal hormones, respectively. In their next series of experiments (70) they bilaterally sympathectomized ten normal adult female rats at the level of the renal arteries. The records of the Achilles reflex time following recovery showed no oestrous variations. Their conclusion was that the hormonal control of the higher nerve centers takes place by means of a mechanism operated by the sympathetic. In a recent experiment on humans, Herren (68) attempted to reproduce the results obtained on normal rats, by recording Achilles and patellar reflex times during the premenstrual and postmenstrual periods. In four of his five subjects there was no change in reflex time associated with changes in hormone level. This result he attributed to the possibility that changes in the hormone level are not of equal physiological importance in lower animals and humans, and also that the human, by virtue of a more complex nervous structure, is not so much a " slave to internal or external environment " as are other mammals.

One study has attempted to determine the direct effect of the female sex rhythm upon the sympathetic nervous system. Eufinger and Arntz (44) tested the reactivity of the sympathetic of 30 women. They compared changes in pulse rate accompanying changes in position from horizontal to vertical and *vice versa,* before and after paralysis of the vagus with atropine. Each subject was examined once during the intermenstrual period and on the second day of menstruation. They found in 83.4 per cent of the cases a sympathetic change accompanying menstruation. In 63.4 per cent the change was in the direction of a reduction of the reactivity, while in the remaining

20 per cent there was a marked increase in the excitability of the sympathetic. These results, though suggestive, cannot be regarded as conclusive. In a number of experiments outside the range of this review, pharmacological methods have revealed a menstrual diminution in neurovegetative tone.

Muscle Strength

If we ascend to a higher level of nervous functioning involving voluntary control, we may consider the effects of the female sex rhythm upon muscular strength. In 1903 Bossi (**22**) studied the effects of menstruation on muscle strength measured by means of the Mosso ergograph. He found in the data of the five women who served as subjects a premenstrual weakening, followed in three cases by increased strength during the flow. After the cessation of menstruation the curves rose to a still higher intermenstrual level. These results have not been substantiated by later work. Almost a decade after Bossi's publication, came two contributions to the study of menstrual cycle effects on strength of grip. Schmotkin (**130**), taking dynamometer records on eight healthy women over an interval of three months, obtained negative results. He found as large fluctuations in the intermenstrual period as in the menstrual period. About the same time Viville (**149**), working with 41 gynecological patients, also found no characteristic menstrual effect upon dynamometer performance. In 1923 Moore and Barker (**103**) reported an investigation of this problem using the Martin spring balance dynamometer. They studied eight muscle groups, involving arms, shoulders, and thighs. They carried on their investigations over a period of 20 months, during which time they were able to observe 30 subjects for from two to twelve cycles each. They made composite smoothed curves for each subject in order to eliminate the masking effect of daily fluctuations. An analysis of these curves revealed a gradual rise from the beginning of the intermenstrual period to a maximum just before menstruation, followed by a depression during menstruation. They also noted a short postmenstrual period of increased efficiency. It is possible that the premenstrual increase in muscular efficiency is related to the increased muscle tonus which is symptomatic of parathyroid deficiency. Bell (**17**) and others have noted a premenstrual drop in calcium content of the systemic blood associated with excretion of calcium salts in the menstrual discharge. Porges and Adlersburg (**118**) have recently adduced indirect evidence of a premenstrual reduction of blood

calcium in the increased excitability of the neuromuscular system. They applied galvanic stimulation to the left ulnar nerve in 11 women. The tests were made either on the first day or one or two days before menstruation. As a control, tests were also made one or more days after the cessation of the flow. They assert an increased galvanic irritability in the premenstrual or early menstrual period. In view of the irregularities in their tables, the reviewer does not regard this conclusion as thoroughly borne out by their data. All we are justified in affirming is their discovery of a tetanoid tendency which requires further experimenting for confirmation. A preliminary announcement by Katz (**83**) of results as yet unpublished fails to reveal any relationship between muscle tonus and the menstrual cycle in a group of psychotics.

"Spontaneous" Activity

A simple form of behavior is the "spontaneous" activity in which an animal indulges. The white rat has proved a good subject in studies of this sort because of the spontaneous rhythms of activity it displays. In 1923 Wang (**154**) attempted to find out whether or not these were affected by the oestrous cycle. He measured the daily activity of a group of male and female rats of from 20 to 70 days in age by means of automatically recording cyclometers connected with activity cages. He found that only the females which had reached sexual maturity showed regular activity cycles. On the basis of observations of 192 activity cycles obtained from a total of 14 females, he found the average length to be 4.3 days and the peak of activity to coincide with the oestrous phase as determined by vaginal smears. A little later Slonaker (**133, 134**) was able to confirm Wang's results using similar methods. The peaks of activity in sexually mature females coincided with oestrus and recurred with great regularity every fourth day. In older females the interval between the peaks of activity was lengthened as menopause approached. A rhythm of activity corresponding to the oestrous rhythm was not shown by males or by ovariectomized females, although it persisted after hysterectomy. Slonaker cited these facts as proving the dependence of the oestrous rhythm on the ovary. About the same time Hitchcock (**74**), comparing the activity of male and female rats, also reported the cyclic nature of the activity of the female as contrasted with irregular fluctuations in the male activity curves. Durrant's (**38**) negative results following hysterectomy lend support to Slonaker's thesis. In further confirmation of this view-

point we have the work of Hoskins (77) and more recently of Richter (122), who found a striking reduction in activity following castration. Moreover, the converse was reported by Bugbee and Simond (23), who were able to induce artificial heat periods in a spayed rat by injection of ovarian follicle hormone. During these periods the voluntary activity equaled that of an unspayed control rat. It is interesting in this connection that Durrant (39), working with mature *normal* female rats, obtained negative results with injections of ovarian extract. Wang, Richter, and Guttmacher (155), in a study of the effect of ovarian transplants on the voluntary activity of *male* castrated rats, presented evidence of activity cycles in a little more than half their subjects following the treatment. In recent experiments Richter and Wislocki (123) have shown the rôle of the pituitary in activity rhythms. Complete extirpation resulted in total inactivity with suppression of the oestrous cycle. Vaginal smears showed the typical dioestrous picture. Extirpation of only the posterior lobes had no effect on the oestrous cycle as revealed by either activity or smears. Furthermore, by sectioning the pituitary stalk Richter (122) has caused a prolongation of the activity cycle. This result suggested to him that while the reduced amount of pituitary secretion in the blood after section of the stalk is insufficient to bring about a full response of the ovary with ovulation every 4.5 days, it may be sufficient to produce it as a summation effect every second, third, or fourth cycle. These results, in confirmation of the endocrine studies referred to in a preceding section, show the necessity of the anterior pituitary in activating the ovary.

Strength of the Sex Drive

Spontaneous activity may sometimes indicate the strength of the sex drive. A more specific measurement of this drive has been made in a series of studies on the albino rat under the direction of Warden. The method used was the Columbia Obstruction method, in which the strength of the sex drive is measured in terms of the number of times an animal will cross, approach, or make contact with an electric grid separating him from an incentive animal of the opposite sex. In the first of these studies Warner (156) correlated the strength of the drive with the oestrous stages of the animal as determined by vaginal smears. He found a decided difference in behavior due to the oestrous cycle. Animals in oestrus crossed on the average more than ten times as often as those in dioestrus. Moreover, animals in dioestrus not only showed little tendency to cross but were not inclined to approach

or make contact with the grid more than once. The onset of heightened sexual activity was rather abrupt and corresponded with the very early part of the cornified stage in the vaginal smears. The transition out of oestrus, on the other hand, appeared to be gradual with regard to both behavior and the histological character of the smears. This rhythm in sexual desire was in contrast with the sexual behavior of the male, which varied directly with the time interval between sexual satisfaction and the test up to an interval of 24 hours, after which the drive showed no further increase. Another interesting sex difference was that the female in oestrus made more crosses to an empty incentive compartment than did the female in dioestrus to a male. A male rat at the height of sexual activity, on the other hand, would cross less often to an empty compartment than would a male in any other condition to a female in oestrus. On the basis of these findings, the author suggested that sex activity is initiated more by the external stimulus situation in the male than in the female, and more by the internal organic condition in the female than in the male.

Further light is thrown on this difference by Jenkins' (**81**) study on the effect of segregation on the sex drive in the white rat. Using the same techniques, she found that a 35-day period of sexual segregation did not decrease the drive of the female in oestrus, although the same amount of segregation decreased the drive in the male. These results also point to the relatively greater importance of internal conditions in the female as compared with the male.

A third study from the Columbia laboratory, by Nissen (**109**), determined the rôle played by the gonads in the sex drive of the albino rat. In females he found that ovariectomy caused a marked reduction in the Obstruction Box scores. Since the sex drive can be thus diminished through the complete removal of ovarian hormones, Nissen suggests that the decreased sexual responsiveness in the female in normal dioestrus is largely due to the absence of the necessary ovarian stimulus rather than to the presence of an inhibitory hormone. It seems to the reviewer that decrease of the female sex drive through removal of the hormonal factors does not preclude the possibility that under normal conditions the drive depends upon delicate balances between the various ovarian components and can accordingly be decreased under the influence of the appropriate inhibitory hormone. The latter interpretation is more consistent with the general trend of results in this field. Nissen was able almost completely to overcome the effect of ovarian extirpation by injecting the rats with placental extract (cow) prepared for the follicular hormone.

This is in accord with numerous injection studies outside the limits of this review. For further discussion of the effects of castration on the behavior of mammals the reader is referred to the recent literature review by Commins and Stone (**28**).

Motor Coördination

If menstruation is a period of supposed reduction of physical and mental powers, one would expect a woman's motor control to show impairment. Fearing (**47**) attacked this problem in the field of static equilibrium. He analyzed nine complete cycles of one woman. Amount of sway was measured by the Miles ataxiameter for the Romberg with heels and toes touching, and for a 45° position. Both positions showed a decrease in the amount of total sway at the beginning of each menstrual cycle. From the second week of the cycle until the beginning of the next there was a gradual increase in the amount of sway. During the premenstrual period the general tension and restlessness became so great as to obscure practice. The author regards the restlessness as a distraction of the attention from the necessary kinesthetic cues. It seems more likely, however, that both the restlessness and the distraction are effects of underlying endocrine disturbances. Schaare (**129**) worked on the related problem of menstrual cycle changes in the accuracy of the perception of position from different angles of a tilting chair. Eleven women and one man were studied over a period of from three to nine months, and further comparisons were made with the performances of five men reported in the literature. The results showed no noteworthy sex differences and no rhythmic changes parallel with the menstrual cycle. More recent experimenting with the same technique has led Kleinknecht and Schaare (**87**) to a recognition of the importance of individual differences in the function studied. Of the 11 women, three showed a decrease in errors while four showed an increase during menstruation. No consistent changes were found for the remaining subjects. They conclude that owing to the wide individual differences no generalization is possible with regard to menstrual changes.

A function that makes very great demands on motor coördination is walking a tight-wire. This has been studied with special emphasis on the effects of the menstrual cycle by Johnson (**82**). 34 women without previous skill in tight-wire walking were tested three times a week for a period of three months. According to the experimenter, there was a marked impairment of efficiency at the menstrual phase of the cycle, followed by a relatively rapid improvement with

a peak at about the 11th day after the end of the period. This rise is attributed to the "period of human oestrus." In evaluating these results we must again observe the lack of a control group, the incompleteness of the data, and the lack of adequate statistical treatment. Moreover, an analysis of his curves reveals drops other than the menstrual drops, that are left unexplained. We are forced, then, to repeat our previous caution to await further experiments along this line before coming to a final conclusion.

Motor Learning

There have been a few experiments on the relationship between changes due to the female sex cycle and maze learning in the white rat. One by Macht and Hyndman (**99**) compared the effects of injections of the blood serum of menstruating and non-menstruating women. A marked lowering of efficiency followed the injection from menstruating women, while either no effects or a slight lowering resulted from normal blood serum. Another by Ball (**12**) determined the effect of oestrus on the learning ability of the white rat. She divided 32 young mature animals into an experimental and a control group. Half were trained during the first part of the oestrous phase of the cycle. Each of the other half was paired with one of the first group and was run as a control. There were no significant differences in the speed of learning of the two groups. The author pointed out the possibility that the increased activity observed during oestrus, which would have a facilitating effect, might be neutralized by the inhibiting effect produced by the relatively inappropriate incentive of food. She also mentioned the more obvious possibility that the learning process might be unaffected by oestrus.

Psychomotor Efficiency

Turning from a consideration of such essentially motor performances, we find relatively few studies on human efficiency in the execution of psychomotor tasks as affected by the menstrual cycle. In 1909 Voitsechovsky (Wyczoikowski) (**150**)* made a pioneer study in this field. Six women between the ages of 18 and 30 served as subjects in this experiment. In addition to physiological measures of blood pressure and pulse, daily records were taken of simple and choice reaction time, continuous free association, Vaschida test for attention, Kraepelin arithmetic test, digit span, and memory

* The reviewer wishes to express her gratitude to Dr. G. H. S. Razran for translating this monograph from the Russian.

for discrete words. The author's curves clearly show a premenstrual rise in blood pressure but no consistent evidence of such a rise in pulse rate. While simple reaction time shows marked and irregular fluctuations which cannot be correlated with the menstrual cycle, choice reaction shows a definite and consistent increase in time and variability during the flow. The author reported that the stream of free associations was also retarded during menstruation, and attention and arithmetical efficiency performances were characterized by an increase in errors just before and during the period. The menstrual cycle seemed to have no effect on memory span. The author interpreted her results as cortical effects resulting from periodic fluctuations of metabolism. In spite of careful technique in the administration of the tests, it is difficult to evaluate her results, since no controls were used and since the results were open to the effects of suggestion due to the subjects' knowledge of the purpose of the experiment.

Hollingworth (**76**) further opened up this field to scientific investigation by her work in 1914. She included a review of the few studies that had sporadically appeared before that time. In her own study she used a series of tests of motor and mental ability including tapping, motor fatigability, steadiness, color naming, opposites, and learning typewriting. They were administered daily under uniform conditions to a group of six women and two men over four separate series of months. An analysis of her curves failed to reveal variations that could be attributed to menstruation. The curves obtained on the men subjects from a chance series of 28 days were similar to those of the women. These results were essentially confirmed in a more extensive experiment with more subjects. On the basis of these results, the author concluded that the menstrual function does not constitute a work handicap in the normal woman. Although this experimenter introduced the necessary external controls, she did not allow for the internal variable of compensatory effort that may have made it possible for her subjects to overcome any disadvantage of the menstrual condition for the brief duration of these tests. A measure of the cost to the organism in terms of oxygen consumption might have revealed the effect of increased effort.

A later investigation in the same field was attempted by Eagleson (**40**). She combined physiological measures of blood pressure, muscular coördination, and mental efficiency. Her selection of tests included steadiness, tapping, color naming, letter-symbol substitution, reaction to an automobile horn, and blood pressure changes.

She had as her subjects three mature women and one girl of 12 years, 11 months, who had not menstruated. Five cycles were covered for two subjects and four for the third. All but one had knowledge of the purpose of the experiment. Her procedure was to take an initial series of pulse and blood pressure readings. Then came the tests, separated from one another by a series of four blood pressure readings. The klaxon was the final stimulus given. In her data she sees evidence of a monthly rhythm in pulse and blood pressure, with a rise three or four days before the beginning of the period and a greater rise the first day, followed by a gradual fall the second or third day and diminished pressure for the next three or four days. There was a general tendency toward a rise from the 14th to the 18th day counting from the first day of the period. The effect of the tests on the blood pressure was negligible. The results for the tests showed no definite rhythm except steadiness, which decreased three to four days before, increased the day before and through the period, and decreased three to four days later and again between the 14th and 18th days. Realizing that the brevity of her tests might have prevented the appearance of menstrual changes because of compensation, she increased the time on the steadiness and tapping tests, lengthened the substitution test, and introduced an adding test, in the second part of her study. A procedure similar to that in Part I was followed. A period of from one to two months was studied for three subjects. Composite graphs of blood pressure and pulse showed rhythmical tendencies similar to those previously noted. The same changes were also noted in steadiness. Increased speed and force of tapping was conspicuous on the first day of menstruation. The substitution test now showed a tendency toward a premenstrual and menstrual increase in the time curve and a postmenstrual period of maximum efficiency. Adding efficiency was also decreased before the menstrual phase. A third part of the experiment was performed on the immature girl over a two months period. A fortnightly peak was evident in her curves, followed by a low period lasting three or four days. Since the author makes no statistical analysis of her curves, we are left in ignorance as to the significance of the changes she reports. Furthermore, even if they do represent real tendencies, we have little basis for judging whether they are to be attributed to the female sex cycle or not owing to the inconclusiveness of the control data.

The year after Eagleson's publication two further contributions appeared on the effect of the menstrual cycle on mental and muscular

efficiency by Sowton and Myers (138) and Bedale (16). In the former study the tests used were spearing, dotting, and number checking. A group of 13 university women and 16 industrial workers were tested daily over a period of from four to six months. The investigators compared the averages of the scores for the days of menstruation and of non-menstruation. With one exception the menstrual period was not accompanied by decreased efficiency of performance. In some cases there was even a dynamogenic effect. The authors point out the inadequacy of singling out the menstrual period for comparison with the remainder of the cycle. The lumping together of pre-, post-, and intermenstrual phases which this entails may mask true periodic changes. This criticism applies to other studies on menstrual cycle effects on behavior. Each phase of the cycle deserves separate analysis and comparison with the others.

The second paper in the joint publication was the report of an experiment by Bedale on the relation of menstruation to general functional efficiency. This represented an intensive study of a single subject over a period of 92 consecutive days. "Functional efficiency" was measured by rectal temperature, basal metabolism, vital capacity, muscle tone, and maximum physical effort on an ergometer operated by hand levers. The results showed a slight tendency toward a periodic heightening in these functions late in the intermenstrual phase and a reduction below the average shortly before or at the onset of menstruation. These fluctuations are in the same direction as the greater number of those reported for similar physiological functions above. Further results on respiration, blood pressure, and pulse rate as influenced by work showed the same kind of variability. On the basis of these results, the author concludes that work during menstruation is performed at a lower physiological cost than at other times because the vital processes are at low ebb during that period. Further work along this line on more subjects is necessary before we can regard this conclusion as validated.

In view of the few studies and conflicting results, any attempt to summarize the work on psychomotor efficiency would be premature.

Affective Reactions

A recent experiment is reported by Freund (53), who presents the interesting theory that the real difference between the menstrual and the intermenstrual periods is a difference in mental "satiability." He criticizes previous work on the ground that the tests used set too definite goals, which served as incentives to overcome the possible decrease in capacity. He works on the assumption that if definite

goals are not set the increased satiability will be easily revealed. He selected two sets of tasks involving precision and care of detail; *e.g.*, counting beads which had to be drawn from a boxful with a needle, or drawing an intricate embroidery design on millimeter paper. The general content was similar but the mental set was different. The "satiation" series was without limit; S was instructed that she could stop as soon as she had had enough: while in the "performance" series a definite end was set. The subjects were 14 women patients in a gynecological clinic, who, nevertheless, had normal menstruation. They were divided into two groups to equalize the adaptation effect. He found that every subject showed a decided increase in the mental satiation during menstruation, which was not accompanied by deterioration of performance. The work speed showed rather a tendency to a slight increase during menstruation and the work quality remained the same.

These results suggest the possibility that the most significant cyclic variations may lie in the affective rather than the intellectual spheres. There is practically no experimental work on the effect of the menstrual cycle on feeling and emotion. An approach to this problem is through a study of the galvanic skin reflex. Prideaux (120) discovered in a series of experiments on two subjects that the galvanic skin reflex was diminished during menstruation. These observations, however, were only incidentally made in an investigation having another purpose. There have been no systematic studies on menstrual cycle variations in the galvanic skin reflex. The writer, in collaboration with J. P. Seward, Jr., is at present engaged in an experimental investigation of the effect of the menstrual cycle on reactions to faradic stimulation. The vaginal smear technique is being used for the purpose of identifying the phases of the cycle.

Another line of attack on the problem of the emotional concomitants of the menstrual cycle is by means of the questionary. Owing to the unreliability of this method, the results obtained thereby are not comparable with those of the experiments that have been reported. Conklin, Byrom and Knips' (29) study by the use of this method has sufficient psychological bearing to warrant inclusion in our review. They devised a questionary designed to determine tendencies toward introversion or extroversion, which they administered to 1,568 women. They found that the general reaction to the period was introvertive. For the most part, questionary studies have been sociological, dealing with sexual hygiene. They have attempted to determine the periodicity of sexual desire and to bring it into relationship with the

menstrual cycle. Most of these authorities find that in the majority of women increased sexual desire is reported either just before, after, or before and after the hemorrhage. This bimodal curve is difficult to explain in terms of homology with the oestrus in lower mammals. In a recent paper, Tinklepaugh (**143**) has suggested that the premenstrual hyperaffectivity is not primarily sexual in nature while the postmenstrual rise in sexual desire corresponds to biological oestrus.

The psychopathology of the menstrual function is a subject of special interest to the psychiatrist, but lies beyond the scope of this review. Recent trends toward psychoanalytic interpretation have been outlined by Chadwick (**25**).

The experimental study of the psychological accompaniments of the menstrual cycle affords a rich and practically untouched field for scientific investigation both from the standpoint of theoretical interest and practical application. Moreover, workers in other lines of psychology who find menstrual cycle changes an extraneous variable should know to what extent they operate in order to control them. Psychological research of the future will be greatly enriched through the development of this important field.

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THE PERSONALITY ADJUSTMENTS OF ONLY CHILDREN *

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The increasing interest in the influence of order of birth on intellectual, physical, and behavioral growth is well manifested by the recent reviews compiled by Thurstone and Jenkins (**63**) and by Jones (**36**). The extensity of these surveys appears to justify closer consideration of the more specialized aspects of this general problem.

While each ordinal position has been identified as the origin of one effect or another, the family situation which by general consent has been thought to be most productive of aberrance is that of the only child. Popular authors and trained observers alike have been generous in attributing various abnormalities to the malign influences exerted by the family environment within which the only child develops. Some have sought to show a correlation between absence of siblings and physical and intellectual inferiority. However, most of the speculation and research concerning the effect of 'onliness' has centered around the behavioral traits of the only child.

Theorists have been in almost complete accord in their observations concerning the maladroit adjustments to be expected of a child having no brothers or sisters. Beginning with the much-quoted Stanley Hall, who is said to have considered being an only child a disease in itself, there has followed an imposing succession of advisers and counselors armed with manifold admonitions for the child raised without benefit of siblings. Blanton (**5**), Groves (**29**), Neal (**52**), Lehrman (**42**), Bruce (12), Miller (**49**), Ueland (**64**), Walsh (**68**), Gordon (**28**), and others have followed Hall's lead in pointing out the 'handicap' which the only child must combat in his struggle to attain an adequate adjustment.

These observations, based more on intuition than actual investigation, coincide nicely with the theories propounded by the psychoanalytic school. Adler (1), Flügel (**25**), Pfister (**55**), and van der Hoop (**65**) have with characteristic ingenuity described the unusual 'dangers' implicit within the one-child family 'constellation' and

* The writer is indebted to Dr. E. S. Conklin who suggested this review and assisted in its preparation.

Brill (11), Lay (40), Wexberg (71), and White (72) have voiced their presentiments in a similar tenor. Of course each of these authors has expressed his conclusions in a somewhat different manner but the general purport of their writings achieves a pessimistic unanimity.

However, it is not necessary that one's conclusions in regard to the only-child situation be directed by theory alone as there has been accumulating during recent years an impressive array of clinical and non-clinical data concerning the supposed psychopathology of the only child.

Historically speaking, Friedjung (26) was perhaps the first to contribute pertinent data from clinical experience. As quoted by Thurstone and Jenkins he found 87 of 100 only children in his practice to be neuropathic while only 31 of 100 children having siblings showed similar traits. Fear, capriciousness, constipation, and enuresis were prominent symptoms and ten of the only children were severely neuropathic. In another early paper Kerley (37) reports in a study of feeding difficulties that "the only child is the most frequent offender and the most difficult to handle." Unfortunately he gave no data in support of his statement. Purcell-Guild (56) has reported the probable causes of delinquency among 131 girls held at a juvenile retention home in Chicago. "Only 19 of the 131 girls had neither brothers nor sisters." Breckenridge and Abbott (10) found 30 only children in a group of 584 delinquent boys. Neither of these studies gives any intimation as to the incidence of only children which might have been expected in a comparable control group.

Burt (15) found only children to comprise 12.2 per cent of a delinquent group under his observation and only 1.7 per cent of a comparable non-delinquent group. However, Slawson (59) in a study of delinquent boys in New York State found but 4.5 per cent to be only children as compared to the 7 per cent which Burdge (14) reports in a group of 145,000 boys not delinquent. In Merrill's study (48) of 236 cases in juvenile court practice the average size of families was found to be five and the only child was not a typical case. Conversely, Armstrong (2a) reports a larger proportion of only children among the 660 runaway boys whom he studied than would be expected from Burdge's data.

Bellerose (3) studied groups of only, youngest, oldest, and middle children, each group comprised of 24 children referred to a behavior clinic. There was a slightly greater occurrence of food fads and

temper spells among the only children but in general they were not unique. Chadwick (19), after spending six months in a nursery school, concluded that only children are more apt to be "undeveloped, almost inanimate" but her conclusions appear to be purely anecdotal. De Lima (21), while taking care of 16 only children, found it "almost impossible to be intelligent with them" because of their dependence, acquisitiveness, lack of empathy, and ill-formed social adjustments. Reynolds (58) recorded the birth order of 400 consecutive cases in a Massachusetts clinic for problem children. Of these 11.8 per cent were only children.

Wile and Noetzel (73) studied the records of 500 children in a large hospital and 355 case records of hospitalized adults. They classified these subjects as explosive, withdrawn, enuretic, or delinquent and, having segregated them on the basis of birth order, concluded that "Ordinal position is not an especially significant factor in fixing a personality type or in establishing definite forms of difficulty in adjustment or in determining dominant attitudes and responses in human relations."

Ward (69) has reported conclusions drawn from the case histories of 100 only children living at home with both parents. She found these children to be notably younger than clinic children as a whole but very much like them in their problem behavior except for their manifestation of stealing, lying, and truancy. When compared to a control group of three-child families the only children showed a higher percentage of restlessness and overactivity, crying, nail-biting, and school difficulties. It is unfortunate that the case histories from which the data were taken were secured from such a widely scattered group of social workers.

Levy's recent study (44) of the behavior problems of 785 Chicago school children is one of the most comprehensive of those done with clinical populations. He compared the incidence of only children among a large group of clinic cases and among a control group of 35,000 non-problem children and concluded that "the distribution of children's behavior problems appears to be for the most part independent of size of family." However, he did find a tendency toward maladjustment in a group of only children of rich parents. Parsley (54) compared 381 delinquent girls with a like number of the same age and nationality who were not delinquent. The delinquents came from smaller families than did the non-delinquents and only children were more frequent among them.

These studies present an ambiguous description of the cause-and-

effect relationship between lack of siblings and behavioral abnormality. However, the negative results reached by most of the more recent investigators at least call into question the direct correlation assumed to exist between these two variables. It is perhaps not surprising that no definite conclusion has been attained by the use of the admittedly rough measures employed in these investigations. As Hollingworth (**32**) has pointed out "Psychiatric reports are likely to represent undue clinical generalization through the fact that only abnormal cases are likely to come to clinical attention. We require similar data on only children not coming to clinics or to psychiatrists before it is possible to make broad assertions concerning the possible advantages or disadvantages of having brothers and sisters."

Kolrausche (**38**) in 1891 reported a study of play-habits of only sons which Jones cites as "one of the earliest compilations of data in this field" from sources other than clinical. Of the 70 only sons whom he studied only 13 per cent participated regularly in school games as opposed to 47 per cent of his control group of boys who had been raised with siblings.

While under Hall's influence at Clark, Bohannon (**8**) undertook a study of 'peculiar and exceptional children,' collecting his data by means of the then-popular questionnaire method. Of the 1,045 such children whose records he obtained 46 were only children. Inspired by this generous proportion, he then compiled (**9**) case histories on 381 only children, the data coming from as many individual sources. He concluded that only children are unmistakably below the average in health and vitality, they are nervous and subject to physical disorders, they enter school late, are irregular in attendance, and are less scholastically successful than average, they have less command of themselves socially than other children, they are commonly precocious, they are selfish but affectionate, and they are given to imaginary companions to compensate for inadequate real companionship.

Following Bohannon's studies interest in 'positional psychology' apparently lapsed and no further non-clinical data were forthcoming until 1923. In that year Korn (**39**), as reported by Murphy and Murphy (**51**), compared the temperament, achievement, and school conduct records of only, oldest, and neither only nor oldest children. There were 25 children in each group and the data were gathered by means of the Mathews emotional stability test, the Otis suggestibility test, teachers' ratings, and school records. The only children appeared to be definitely superior in work habits but were signifi-

cantly less emotionally stable than the other children. They seemed to be just as good mixers as the others but were not as apt to participate actively in athletics. Lehman and Witty (41) in their study of play interests found only children to be only slightly less sociable and Hsia (34) observed that the most sociable children usually have fewer brothers and sisters than the least sociable.

Thurstone and Jenkins quote Hentig and Vierstein's study (31) of incest as indicating a greater prevalence of this perversion among only children. Of 92 incestuous persons studied 9.8 per cent were only children as compared to 4.1 per cent of a control group of 1,660 soldiers. The difference was considered statistically established as significant.

Stuart (61) studied the undesirable emotional outlets of 465 young men, including 81 only children, using Laird's Mental Hygiene tests as a measure. "In no division of the test was there any indication of the number of deviations from normal being dependent upon the number of siblings." Goodenough and Leahy (27) sought to measure the effect of certain family relationships upon the personalities of 293 five-year-old kindergarten children. By use of teachers' ratings they found that in general only children were more aggressive than oldest or intermediate children. They were highly gregarious in social interests, more than ordinarily likely to show instability of mood, were easily excited, and their attention was flighty and distractible.

Blatz and Bott (6) through teachers' ratings surveyed the misdemeanors of a group of kindergarten and grade school children. The 45 only children included had the best records and 62 per cent of them had no misdemeanors whatever. Stratton's study (60) of the relation of fear and anger to primogeniture indicated that among his subjects the first-born, whether only children or not, were inclined to be more irascible.

Fenton (22) took the ratings of Ohio teachers on the character traits of 193 children including 34 only children and found 90 per cent overlapping in all traits. Only children were slightly more apt to be leaders, being somewhat superior in self-confidence. They were more aggressive, happy, and slightly less conscientious in truth. In a further study Fenton gave 512 college students, including 73 only children, 101 questions from the Woodworth questionnaire. On anonymous replies the only children gave more positive answers than did the non-only. When the reports were signed the situation was

just reversed. However, the difference was only a matter of three answers in either case and none of the averages was unusually high.

Busemann (16, 17) studied 400 German children between the ages of ten and seventeen. As indicated by teachers' judgments, in general the children tended to better records in behavior, industry, attention, and deportment the more siblings they had. Only children were found to be characterized by reflectiveness, self-dissatisfaction, wiggliness, and poor school success and deportment. Girls were particularly benefited by siblings. Blonsky's study (7) of 193 Moscow school children led him to conclude that the only child is an 'ultra-character,' either very introverted or extremely extraverted. The 33 only children in his group were nervous, domineering, or else unusually bashful, and disliked by their schoolmates. Buehler (13) notes Czerny (20) and Neter (53) as also finding the only child to be 'nervous.' Machacek and Tremel (45) studied 57 boys raised as only children in Vienna and report them to be unpopular and socially ungifted.

Guilford and Worcester (30) have reported the personality traits of 21 only and 141 other eighth-grade children whom they studied. The only children of their group were definitely superior in personal orderliness, cleanliness, initiative, self-control, industry, truthfulness, dependability, and courtesy. They were somewhat less superior in coöperation and fairness and approximately equal to the non-only children in voluntary participation in extra-curricular activities. Worcester (75) subsequently studied the schoolroom attitudes of 99 only children recruited from eleven school grades. Taking his evidence from report cards, he found the only children to be uniformly superior in courtesy, play habits, and obedience although the superiority decreased in the higher grades. In a study of happiness among adult students Watson (70) found that the 26 only children who took his test had not grown up to be unhappy adults. There was a slight tendency in the other direction.

Vetter (66), in measuring atypicality and typicality in views on various questions, found more than a chance number of the 21 only children among his subjects in the atypical, reactionary, and radical groups. "It somehow seems peculiarly fit that the more extreme positions should contain more only children; they have a reputation for more unusual emotional development." In a more recent study Vetter and Green (67) sent questionnaires to 600 members of the American Association for the Advancement of Atheism. Of the 350 replies 9 per cent were from only children, inconsiderable when com-

pared to the 36 per cent coming from oldest children. Thurstone and Thurstone (**62**) applied their neurotic inventory to a group of college students of whom 99 had been raised without siblings. They were unable to make any differentiation into best-adjusted or most-neurotic groups on the basis of 'onliness.' Lester and Barnette (**43**) gave the same scale to 95 freshmen women at the University of Buffalo. "There seems to be no marked tendency for the only child to be adjusted or unadjusted." Angell (**2**) from a study at the University of Michigan concluded that although his results did not justify any degree of assurance "it appears probable that only children have a slight tendency to gravitate toward the maladjusted classes."

Maller (**46**) reported a significant correlation between children's coöperativeness and the number of brothers and sisters. In a later investigation (**47**) he studied 802 children from varying social and economic levels. The only children in this group he found to be "above average in intelligence, moral knowledge, cultural background, and honesty. They are just average in coöperativeness and persistence and below average in inhibition and ratings by teachers and classmates." Hooker(**33**) paired a group of 30 grade school only children with a group of siblings on the basis of school grades, sex, age, nationality, family organization, and intelligence quotient. To these 30 pairs he gave 18 questions drawn mainly from the Terman revision of the Woodworth-Mathews PD sheet. He also obtained teachers' ratings of the children. His results showed the only children to be better adjusted and less inclined to be neglectful, forgetful, irresponsible, sullen, nervous, or dishonest. Hurlock and Burstein (**35**) gave a questionnaire concerning imaginary playmates to 701 high school and college students. Size of family did not influence the creation of imaginary companions among their subjects and the only children were not found to be more subject to imaginary companions than were those who had brothers or sisters.

Campbell (**18**) gave the Bernreuter Personality Inventory and Cason's Annoyance test to 200 college students in a comparative study of only and intermediate children. The subjects were paired for high school grade record, college entrance examination score, sex, and class in the university. The differences in mean scores of the two groups on the four measures of the Bernreuter inventory indicated a slightly greater incidence of atypicality among the only children, particularly the girls, but in no case was the critical ratio significant. The variability of the scores of the only child group on each of these four scales

was somewhat greater than that of the paired group. Bender (4) also noted a wider variation in the scores of his only child group when comparing their scores on Allport's ascendance-submission scale to those of a group of intermediate children. In his study there was a tendency for the only child to be slightly above the mean in ascendance and for the intermediate to be somewhat submissive.

Witty (74) has recently presented a preliminary report of a study of 153 only children of the chronological age five. On ratings of courtesy, truthfulness, self-control, initiative, orderliness, coöperation, and dependability the number of the only children reaching or exceeding the median of the control group was in every instance somewhat greater than 50 per cent. The incidence of speech defects, headaches, night terrors, enuresis, excessive timidity, and defecation irregularities was not unusually high among the only children although a relatively large per cent of them were subject to specific fears. It should be noted that these children came from families of better than average socio-economic status.

It is interesting to observe how theory and research have drawn apart in their conclusions as to the efficacy of the only-child constellation as a personality determiner. In general, theory appears to have changed very little since Hall's day, as indicated by the forecast of "double danger for the only child" appearing as recently as 1930 (57). Research (excluding the German contributions), both clinical and non-clinical, has given increasingly little support to this armchair viewpoint. It is evident from the disparities which exist in the experimental data that the issue is still not entirely settled. The difficulty in applying adequate controls in studies of this nature makes an exact evaluation of the data gained from them a dubious procedure. However, the skeptical attitude expressed by Fenton (23, 24) and Morgan (50) appears to be well justified. 'Onliness' *per se* is not the environmental specter so widely assumed. Whatever rôle the mere presence or absence of siblings may play in the development of personality, its importance is certainly not crucial.

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TROLAND, LEONARD T. *The Principles of Psychophysiology.* Vol. III, Cerebration and Action. New York: D. Van Nostrand Company, 1932. Pp. 446+xxiv.

After the excellent reputation achieved by Volume II in this series the psychological public will be eager to appraise the above volume now at hand. It will be all the more valuable since, owing to the unfortunate death of the author, some doubt exists concerning the ultimate publication of the fourth volume in the series on "the philosophical developments of psychophysiological facts and principles: an analysis of the ultimate significance of the mind-body relationship, and an attempt to found a new philosophical methodology."

We are told in the preface to this volume that the treatment of *cerebration* embraces "the laws which relate experience or consciousness with the central nerve processes and structures" and that the discussion of *action* encompasses "the laws which associate consciousness with factors in the efferent sector of the response arc, including behavior, or the postures and movements of the organism." In the present state of knowledge the author has been able to present fewer experimental data under the last heading and still fewer facts in connection with the former heading than he was able to embody in the volume on *sensation.* Since about the first two-thirds of the present volume, therefore, deals with cerebration and especially with the cerebral cortex in which the "direct determinants of the introspective consciousness" are to be found, hypotheses predominate and the author is forced to select his own position on speculative grounds. Although the author confesses that in this treatment there is a close approximation to the *Gestalt* point of view, he denies any influence from that direction because he arrived at his own conclusions independently and prior to the publication of the *Gestalt* literature. His main contention against this school is that, like behaviorism, it is too exclusive. He takes a more comprehensive attitude and is quite willing, as he has already shown in Volume II, to consider sensory relationships and other phases of consciousness *per se.* This aim is most emphatically announced when he says: "Let us be courageous and envisage the entire system! If, in the present work, I have approxi-

mated such an ideal—without loss of clarity—I have accomplished one of my principal purposes."

The first five chapters deal with the question of cerebration proceeding from the general psychophysiological problem, then considering special psychophysiological configurations in connection with the various projection areas of the cortex, discussing next more detailed experiential patterns, and ending with a chapter on the physiological substratum for affective experience with a short concluding chapter summarizing conflicting hypotheses on the subject of the cerebral foundations of experience. Throughout this exposition the underlying theme is that for every difference in experience there must be a corresponding difference in the functioning of the cerebral cortex and that the intricacies of both must be at least mathematically equivalent. This central hypotheses is designated as one of *formal identity*.

The specific patterns of afferent neural conduction lead to *neurograms* representing integration, whereas the efferent system emanates from an *incitogram*. Connections are also formed between these two systems during the process of learning.

A large volume of systematic research is condensed in the following sections outlining the complicated paths from sense-organ to cortical projection. Numerous diagrams facilitate this presentation of material. There is also a unique and simple schematic presentation of the chemico-electrical explanation of neural conduction. One of the most constructive sections deals with the allocation of the "restricted determinative region" of consciousness. The author tries to avoid placing it in either hemisphere but finally decides that the frontal and prefrontal association areas afford the best location. The reviewer can not quite see the urgency for all of this labored discussion since the tendency in the more recent work on brain localization is away from a specific locus and towards a more functional hypothesis attached to a great variety of cortical areas.

Of course the author is committed to what he calls a monophasic hypothesis which leads him to a definite region of synergic activity. But the accumulating evidence appears to the reviewer to be increasingly in favor of a hierarchal synthesis. Since this would lead to a discussion between the author and the reviewer which in the nature of the case can not take place and since in any event the issue is highly speculative, the reader is invited to draw his own conclusions from the argument which takes place in the author's final chapter in this section (XXIV).

Equally significant are the author's general principles in regard to the question of the correlates of qualitative experiences. He concludes that, "... *physical structures which are minute below a certain point, yield qualitative, rather than structural differences, on the psychical side.*" This is qualified by the statement that not the microscopic minuteness itself but the concomitantly "great strength of the electromagnetic forces which bind such physical systems together" is operative. Stated again, "*if a configuration of electromagnetic forces is sufficiently intense, or well integrated, the psychical parallel is not a pattern but a quality.*"

From this point on there is a short discussion of the *Gestalt* position with several illustrations leaving room for both the traditional qualitative analysis and the configurational description with the possibility of a critical liminal value between the two stages of analysis. Shorter sections are occupied in the presentation of physiological bases for attention, subconsciousness, imagery, memory, imagination, and recall.

The nature and correlates of affective experience is to the author the most important problem in cerebral psychophysiology. He emerges from this section with three systems: the *nociceptive,* the *neutroceptive,* and the *beneceptive.* The first refers to the neural mechanism for the reception and transmission of pain, usually affiliated with unpleasantness; the second is concerned with the reception and transmission of those sensory qualities that are affectively uncolored; and the third with those that are typically beneficial and generally pleasant. The first and last function in the direction of inhibiting and facilitating, respectively, the concomitant synergic conduction in the cortex, a correlation laid down by heredity because of biological utility. In addition, however, a *retroflex* response is called into play as the result of beneficial or harmful stimulation, which not only explains the resultant affective turn in consciousness but, in terms of its *rate* of conductive facilitation or depression, determines the degree of pleasantness or unpleasantness. This reflex control is referred to the diencephalon. In connection with this system the effects of drugs, endocrine secretions, and hormones are discussed.

The last large division of the book deals with the psychophysiological relationships between experience and processes of the efferent arc. After a preliminary section dealing with the traditional characterization of volitional action the kinaesthetic quality is made to do significant service as the specific sensory concomitant of efferent "effects." The functions of motor control in all parts of the higher

centers are described in some detail and in a fairly orthodox fashion; so are the reflex actions and their centers in the human being. There is an interesting section on appetition which is described as a series of random movements not completely unsystematic but unpredictable because of their diversified character. They are aroused by certain stimuli or physical conditions and produce a condition of unrest. Instinctive reactions, voluntary and volitional responses, the reaction experiment, and the mechanism of insight are accorded a rather hurried treatment.

The book closes with a heterogeneous collection of topics dealing with the psychophysiology of emotion, purpose, habit, and the selection of action tendencies. There are repeated references to the author's "Fundamentals of Human Motivation." No major advance is made in the discussion of this section. Volitional action is placed on a deterministic basis and the hedonistic theory, familiar to the readers of the author's other book just quoted, is reëmphasized. At the end of the volume an unalphabetized bibliography of 272 titles, numbered consecutively from the previous volumes, is added. There is also a joint index of subjects and authors.

The book will undoubtedly take its place among the serious compendia of psychological and physiological facts. It is compactly written but carries an abundance of rather specialized terminology and formal statements. Even the expert in this particular field of cerebral and other neurological correlates may find, the reviewer fancies, repeated reading necessary. But in no other work on the subject are there so many researches coördinated and interpreted with expert understanding. One gathers from the reading of any section, except perhaps the last, the impression that the author had an unusual facility for scholarly assimilation and creative thinking. If the latter has been carried to a fault in this volume it is only because experimental data, especially on the neurological side, are so scarce and speculation is so rife. Besides, even at the present stage of knowledge the relation of mind to body is almost as much fraught with logical difficulties as in the days when Descartes made one of the earliest systematic proposals in this direction.

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HEIDBREDER, E. *Seven Psychologies.* New York: Century, 1933. Pp. viii+450.

Seven Psychologies was written with the interest of the elementary student in mind, for the purpose of affording him some insight into

the nature of modern psychological trends and some foothold in what he must often judge, from a beginner's acquaintance, to be a most slippery field of controversy.

The first pages of the book consider the general questions, why widely divergent points of view should arise in a science, and whether the tendency toward theoretical disagreement is on the whole deplorable. The author offers the commendable observation that speculation is paralyzed only by either of two conditions—the presence of no factual evidence whatsoever, or the presence of a complete structure of fact. Between these extremes, where psychology now stands, we may expect theorizing to be—as it is—enthusiastic. Nor, again, does she hold that this inevitable ebullience of the speculative intellect works any ultimate harm; for the philosophy that condemns it with the canon that thought shall proceed only so far as knowledge gives support, neglects the circumstance that data do not give birth to conclusions, but are themselves the offspring of speculations which demand verification.

Having thus noted the psychological causes of division into schools, the book proceeds to a brief history of pre-scientific psychology. The principal philosophical thinkers who have contributed to the development of psychological notions are treated in order, and the most notable of their doctrines are sketched interestingly and accurately. The scene is thus set for a consideration of the present status of psychology as represented by the outstanding American schools.

The first of the psychologies which Professor Heidbreder describes is that of Titchener. Logically it is shown to precede all other American movements from the fact that not only did it derive directly from the first scientific psychological system of Germany, but also that it stood for so long as the object of reference for those systems which broke from it in protest. The conceptual structure of the psychology of Cornell, as well as the personality of its genius, are well and sympathetically given. The chapter concludes with a keen assessment of the worth of structuralism: its defects were those of too ascetic and self-conscious a contempt for the psychology of common sense, and its unforgettable virtue was that of the strength it contributed to an autonomous science by its bold reliance on empirical, experimental method.

The second chapter deals with William James and his psychology. The *Principles* are outlined, with especial attention to the famous chapters on the Stream of Thought, Habit, and Emotions. The author

brings into prominence James's attitude toward the experimental method—his Yankee respect for fact, reality, and evidence, mingled with his liberal reluctance to see psychology unreasonably confined by too rigid or too segmental an approach. James's influence is made memorable for its effect in broadening and refreshing the psychological point of view and in the extension of both its interests and the material upon which it draws for evidence.

From James it is a short step to Functionalism, the next movement which Professor Heidbreder considers. From the establishment of this school in Dewey's paper on the reflex arc, through the psychology of Dewey and of Carr, there is traced the growing revolt against a psychology of content, and a steady turning to the study of the functioning individual of whom consciousness and behavior, stimulus and response, are but heuristic aspects. It is the thesis of this exposition that Functionalism never quite attained the conceptual maturity of a full-grown school of thought—that is was throughout rather more inspirational than analytic in respect of its theoretical foundations. But during its distinguishable life it managed to incorporate inextricably into psychology the broad and organismic view of mind which is now commonplace doctrine. As a consequence of the functionalist swing from structuralistic dogma, the way was opened to Behaviorism, to an exposition of which the fourth chapter is given.

As proponents of Dynamic Psychology, the author selects Woodworth and McDougall for emphasis. She portrays the school as less revolutionary and polemical (at least in the hands of Woodworth) than the others, but also as unified by a common appreciation of the importance of goal-directed activity and of analysis along the temporally extended dimensions of mental life. This chapter makes clear not only Woodworth's and McDougall's systems, but also the fundamental difference between them—the question of the autonomous development of habit-systems into drives.

An especially well organized chapter is that on the Gestalt school. The history of the movement, from its protest against the atomistic view in relation to certain particular problems to the development and extension of configurational principles over the whole field of psychology, is clearly and engagingly developed. The chapter contains, among other critical estimations, a reply to the objection that Gestalt psychology contains little that was not implicit historically before its emergence. The author holds that the new point of view

at the very least crystallized out of psychology's unconscious certain concerns which are the better for having been brought into the light, and that, beyond this, the school has to its credit more than sufficient experimental accomplishment to justify its claim to novelty.

The last of the schools with which the book deals is that of Psychoanalysis. The author outlines genetically the development of Freud's approach to the problems of insanity and the normal personality and the later revisions of his major theories. The chapter ends with the witty comment that to criticize Freud's system is to fall victim to it, inasmuch as to the consistent Freudian both censure and praise are equally explicable as manifestations of the mechanism of rationalization and need carry no intellectual force.

The book concludes with a section titled "Afterthought," in which Professor Heidbreder performs the task which so many other critics have neglected—that of directing attention to the residual agreement left in the wake of criss-crossing lines of thought. She finds that although the statement that psychology can at present show a strongly united front is hyperbolic, nevertheless many theoretical divergences have served the purpose of revealing agreed-upon facts. Thus Lashley as a behaviorist and Köhler as the critic of behaviorism come soon to much the same premises concerning the essentially transposable property of central nervous processes. Watson and McDougall have in their divergent ways proclaimed and established the absurdity of a rational motive behind behavior. Functionalist and introspectionalist have arrived at a common appreciation of the pervasive influence of sets and determining tendencies. The chapter implies the reasonable hope that these happy meetings can be taken as a prediction of more to come; and that every such convergence upon common ground is not only an achievement but a guarantee for the future as well.

What is to be said, finally, of the book as a total structure? Quite simply, I think, it stands as one of the most finished, bright, and effective expositions that we possess. Professor Heidbreder has done, with wit, insight, and the fluency that distinguishes competence from pedantry, what she hoped to do for the elementary student. She deserves his warmest thanks; and the gratitude will undoubtedly be shared enthusiastically by those who are long since beyond his pedagogical needs.

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HULL, CLARK L. *Hypnosis and Suggestibility: an Experimental Approach.* New York: D. Appleton-Century Co., 1933. Pp. 420.

With the appearance of this book, which is written around the researches of Hull and his associates—being dedicated to the latter—hypnotism has become hypnosis. For the first time we have a book which does not repeat the old hypnotic lore. Instead of quotations or citations of authorities we find details about the methods and results of controlled investigation of several of the classical hypnotic phenomena. On just this account, however, it is a book for the scientist and not for the layman. The benefits of the general method of treating the question are great: it gives hypnosis a status comparable with that of any other psychological problem, inasmuch as quantitative data become available in many different aspects of the subject. The conclusions do not outrun the data. Best of all, it opens up many fields of hypnosis for further experimentation.

The defects of the general treatment are those of a pioneer work in a most difficult field. One doubts that some of the tests employed are just the tests for the purpose—regardless of how well controlled the individual experiments may be. This point will arise later in this review. Then, too, holding to his purpose to present only such data as he thinks strictly scientific, the author limits the subject matter of the book greatly, leaving out, for example, any reference to clinical or forensic problems. More than that, researches of some other psychologists, in case these researches do not fit into Hull's experimental approach, are not even mentioned, notably Klein's work on hypnotically induced dreams and Morgan's and Travis' on criteria of suggestibility. Lastly, by thus limiting his purpose, Hull's explanatory principles are correspondingly narrow, *e.g.*, the theory that hypnosis may be a form of coöperation is not mentioned, nor is there any reference to the Freudian theory.

As to the phenomena of hypnosis Hull, in conformity with laboratory findings since 1923, states that nothing except heightened suggestibility—and, possibly, better memory for long past events—differentiates hypnosis from the waking state. The tall stories of hypnotic ability beyond the normal in sensitivity, perception (*e.g.*, of time intervals), endurance, memory, learning ability, etc., are declared discredited by experimentation. On all these points the reviewer agrees with the author. Yet according to all reports, including Hull's, hypnotic subjects *think* their powers heightened, and uniformly deny any fatigue and pain sensations. Hull is even inclined to believe that the effect of suggested anesthesia "is operative to a

considerable extent on the volitional level, but the fact that both pulse rate and the galvanic skin reaction show very appreciable effects points to some other and more deep-lying mechanism as also operative" (p. 265). As regards post-hypnotic amnesia Hull says, "We arrive at the conclusion that post-hypnotic amnesia is not a primitive physiological mechanism but that, instead, all of the phenomena are probably produced by suggestion in some form" (p. 154). And yet for practical purposes Hull measures the depth of hypnosis by the extent of post-hypnotic amnesia. A neat distinction is made between "one hundred per cent hypnotic amnesia by the method of symbolical reinstatement traditionally employed in this field [*i.e.,* denial of any recollection of trance events] and only 50 per cent amnesia by the relearning method" (p. 133). Catalepsy and *rapport,* which in the older literature were sometimes thought to inhere in the state of hypnosis, are considered by Hull to be results of explicit or tacit suggestion. Although hypnotic or post-hypnotic dissociation (in the sense of "two simultaneous independent conscious processes") is proved non-existent, Hull still considers hypnosis a condition of monoideism, in which "proprioceptive intrusions" are reduced, "sensory competition" is removed. From other statements in the book, also, it is clear that Hull does not dispense with the concept of dissociation. The once marveled at hypnotic control over non-voluntary processes is seen to be only a special form of conditioning, exemplified in Hudgins's bringing about pupillary contraction in a subject who "thought" the word "contract" [after having been conditioned to the sound of the word just before a strong light was thrown into his eyes]. Hull, like most scientific experimenters, finds that normal subjects—as opposed to feebleminded or pathological persons—are the most susceptible to hypnosis.

Thus, backed by very good laboratory experimentation, Hull has brought hypnosis out of the darkness into light. He has given currency to laboratory findings, until now available only in the psychological journals. But some of these problems are exceedingly difficult to investigate; the influence of post-hypnotic suggestion, perhaps, the most difficult of all. Nearly all demonstrators of hypnotic phenomena have been struck with the all but irresistible effect of post-hypnotic suggestions, especially if the suggestions are to be carried out immediately after awaking. For instance, if a person in the trance is told that on awaking he will drink three glasses of water out of a pitcher on the table, his behavior on awaking is almost comically directed toward the water pitcher. Such simple evidence—even without

scientific controls—is not outweighed by a complicated well-controlled nonsense type of investigation in which "The post-hypnotic suggestions by Kellog were that the subject while reading the even-numbered pages of a book would breathe twice as fast as usual, but when reading the odd-numbered pages he would breathe only half as fast as usual" (p. 157). Nor is the common knowledge of post-hypnotic effect to be slighted in favor of Patten's experiment, which depended on amplitude of the waking finger response to words seen before in the trance. This kind of experiment is also lacking in meaning; besides, amplitude of reaction was not part of the suggestion.

The question of employing tests not too well chosen for diagnosing hypnotic conditions becomes critical when Hull uses results of a speed test as a measure of relaxation. On the contrary, if time is to enter into a test for relaxation, the token of relaxation should be negative rather than positive speed. And yet with results based on speed of reaction Hull rejects the relaxation theory as an explanation of hypnosis. He also concludes that hypnosis is quite different from sleep. "The physiological criteria of the knee jerk . . . the psychological tests of voluntary reaction to a signal . . . show the trance to be allied, rather, with the waking state. . . . Thus the extreme lethargic state is not hypnosis but true sleep; only the alert stage is hypnotic" (p. 221). The experimental procedure by means of which hypnosis is differentiated from sleep seems scientific enough; but if Hull and his associates used in their controlled experimentation the method of hypnotizing published in this book, no methodology employed later on in hypnosis to test relaxation could have any weight: the very method of hypnotizing inculcated relaxation as the central idea. In fact, in Hull's report of his method of hypnotizing he over and over again mentions becoming drowsy, getting tired, falling asleep, and being sound asleep. Unless hypnosis as induced by this type of suggestion (in regard to sleep) is an entity independent of those suggestions, it is too much to expect relaxation to be measured by a speed of reaction faster than the normal waking speed. The "sleep" suggestions may not bring on sleep, but they certainly do bring on relaxation. That Hull himself considers hypnosis a kind of relaxation is evident, since he speaks of monoideism as resulting from a "*quiescence* or withdrawal of the stimulation arising from the subject's own symbolic processes" (p. 397, italics mine), and of the subject's symbolic processes remain-

ing "*passive* so far as the particular act suggested is concerned" (italics mine).

A major assumption of the book is that direct or prestige suggestion is different in kind from indirect or perseverational suggestion. An example of the former is falling forward at the experimenter's saying, "You are falling forward"; of the latter, reacting to Binet's progressive lines. Although Hull does not consider the experimental data entirely conclusive, "it may be said with a fair degree of confidence that non-prestige suggestibility differs from hypnosis in at least four respects: (1) its correlation with susceptibility to hypnosis, (2) its relation to scholastic intelligence, (3) the direction of the habituation effects, and (4) the tendency of the hypnotic state to heighten suggestibility" (p. 384). In theoretical criticism of this conclusion one might point out that certainly indirect suggestion, as much as direct suggestion, is a matter of attentional, ideomotor and monoideitic phenomena. Again, Hull's technique of centering the attention on falling forward, on going to sleep, on "I hold in my hand a powerful hypnotic crystal," is no more direct than centering the attention on the increasing lines, on the magnetic properties of the bar, or on the faintest degree of warmth that is perceptible. To the reviewer, this opposing direct to indirect suggestion seems doubtful, and the experiments quoted in proof of this opposition seem inconclusive. Indeed, Hull's methods of hypnotizing as exemplified on page 29, in using the "powerful hypnotic crystal," in simulating in himself a drowsiness induced by gazing at this ball, and in suggesting "You are sound asleep," is nothing if not indirect: if hypnosis has nothing to do with sleep, or even relaxation—as Hull believes.

How is hypnosis to be explained, then? "So far as these data go, the indication is that hypnosis and the ordinary forms of direct verbal suggestibility may significantly be included within a single definition" (p. 78). It is a habit-attention-ideomotor phenomenon. It is monoideism. "Bernheim reduced suggestion substantially to the association of ideas; the view here presented reduces it to the strictly physical basis of the associations between stimuli and responses, ideas becoming purely symbolic acts" (p. 394). "In the case of heterosuggestion and hypnosis, the conditions of attention or monoideism would be fulfilled when, either through the quiescence or the withdrawal of the stimulation arising from the subject's own symbolic processes (ideas), proprioceptive intrusions would be reduced. This removal of sensory competition would give the continuous stimulation emanating from the symbolic processes (ideas)

of the experimenter a kind of right of way to the control of the subject's movements" (p. 399). "This evidence supports the hypothesis long held that ideomotor action is the basis of prestige suggestions" (p. 62). Hull's final word of explanation is that hypnosis is a habit phenomenon. "Thus we find in essential functional agreement the points of view of ideomotor action, monoideism, and attention when they are viewed as habit phenomena" (p. 399). If habit were a good explanation of hypnosis, every person who is willing to be hypnotized and who submits to the procedure a certain number of times should go into the trance. Many subjects, however, report that they are still unaffected after as many as ten attempts to be hypnotized.

Even though the theoretical conclusions of the work are not novel, nor the tests always the most suitable to the purpose, this book must be studied by all psychologists who wish to keep *au courant* with hypnosis. The book is yet to be written that will bring this knowledge to the general public.

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MURCHISON, CARL (Editor). *A Handbook of Child Psychology.* Second Edition Revised. Worcester: Clark Univ. Press, 1933. Pp. xii+956.

Two years after the appearance of *A Handbook of Child Psychology* a second edition revised comes to our desk. Since the possession of this second volume will necessitate the eschewing of the other possible benefits of five "depression" dollars, we shall probably serve our readers best if in our review we appose the revision of the "Handbook" with the original work. The latter we thought suffered by unwarranted emphases and gross gaps or omissions. The topics whose very superficial exploration we had lamented were those of antenatal and postnatal life, the adolescent period, and certain frequent forms of human deviates. For these lacks the "Revision," with its quarter thousand additional pages, almost supererogates. A chapter, 132 pages in length, by Dr. Leonard Carmichael on "The Origin and Prenatal Growth of Behavior" is outstanding both for its comprehensiveness and scholarliness. Since it overlaps considerably the sector contributed by Dr. Karl Pratt on "The Neonate," we are inclined to believe the two might well have been combined. The volume would then be less open to the criticism of lack of

balance; for, as it is, about one-fourth of the total contents are given over simply to getting the human being started on his biologically independent existence. We react not too unfavorably, however, to the emphasis, because there are few sources with the focus desired where any considerable quantity of the psycho-embryological matter has been assembled. Relatively more space for the material than its essential importance would appear to justify seems defensible in cases such as this, where, because of the heavy drafts necessary on the literature of a discipline other than psychology, or in parallel cases where the drafts are upon foreign literatures, the average psychologist's library provides little substitute provender. The assignment of a whole chapter to the topic, eidetic imagery, for instance, we can sanction for this reason, though we do regret the scattering of the material on children's thinking through chapters concerned with learning, children's philosophies, eidetic imagery, language development, etc. This results in an essentially weak or disintegrated presentation of a very important subject.

In contrast with the thorough survey made of the literature concerning the antenatal and infant periods, the account of adolescence seems rather abbreviated. In fact, the whole volume appears to focus on the young child, with illumination decreasing progressively as the areas concerned with older children are entered. This difficulty arises probably partly as a result of a lack of agreement in kind among the units of the volume's frame-work, the skeleton organization being based on a cross-sectional, temporal plan in spots, whereas in others it is based upon categories traditionally employed in describing mental organization. The "Revision," in other words, like the original book, has no very logical organization but may be characterized as covering the major topics around which workers have been grouping their research and thinking.

Among the wholly new chapters in the 1933 edition, in addition to the ones already mentioned, are the following: "Locomotor and Visual-Manual Functions in the First Two Years," "The Measurement of Mental Growth," "Speech pathology," "Sex Differences," and "The Child with Difficulties of Adjustment." The first mentioned is written by Dr. Shirley, and while well presented and excellent in quality, its contents overlap, or are overlapped by, the material in the chapters on "The Origin and Prenatal Growth of Behavior," "The Neonate," and "Learning in Children." Dr. Shirley's division probably could have brought into the array of assembled data more of what was neglected by others had it been extended to cover the

problems of the nature of motor ability and the development of motor skills through the whole of childhood.

Dr. Goodenough has advanced the worth of the "Revision" with her discussion of the measurement of mental growth. She gives most weight to the problems of scaling, of relative scores, and of prediction by means of so-called intelligence tests or tests of general development. The treatment of the fundamental question of mental organization and what it is we are measuring seemed to be, for so basic an issue, somewhat abridged. Mental measurement problems are envisaged largely as those which appertain to what is usually called intelligence, any that might be unique to the so-called special abilities being almost totally ignored. Somewhere in the volume, whether in the Goodenough chapter or not, we are not prepared to say, these need consideration.

Not only has new material been added in the "Revision" but some has been deleted. Chapters present in the first edition not found in the revised are those concerned with the subjects of psychoanalysis, physical growth of children, dreams, plays and games, drawing, and "The Experimental Construction of an Environment Optimal for Mental Growth." The material on eating, sleeping, and elimination receives consideration at the hands of a different author, Dr. Blatz, and in a wider context described as the physiological appetites. Retitled, and to a greater or lesser degree refocussed, are the chapters contributed by Drs. Gesell, M. C. Jones, Lewin, and Terman and Burks. The latter authors also supply a much more inclusive bibliography on gifted children than was given originally.

Most of the scientists contributing to the "Handbook" have evidently conceived of their task as one of providing a comprehensive survey of the literature in an assigned territory. When issues have been viewed from several perspectives or present patterns in the description of which all do not agree these differences have usually been made clear. Little effort has been expended, however, in attempting to reconcile findings or account for differences. We regret, though we celebrate that excoriations and philippics have not appeared, that more of the contributors did not follow a plan similar to that adopted by Dr. H. E. Jones in his chapter on "Birth Order." He points out first, in masterly fashion, the pitfalls into which investigators of the variable have stumbled and then is in a position to suggest appropriate cautions with respect to the interpretations of various findings. While having some misgivings concerning the advisability of giving over a whole chapter to the single variable,

birth order, we still consider the discussion by Dr. Jones one of the most valuable in the volume.

Although, as we have just indicated, most of the presentations are in the nature of comprehensive surveys, there are some which are essentially the exposition of a single man's views on a certain subject. Among these are the chapters by Dr. Gesell on "Maturation and Patterning of Behavior," by Dr. Piaget on "Children's Philosophies," Dr. Lewin on "Environmental Forces," and Dr. Blatz on "The Physiological Appetites." In the main these one-man chapters have emphasized some of the more fundamental theoretical considerations and, while the treatments have generally seemed profound, a more explicit exposition of contrasting or differing views seems desirable for a volume of the type of the "Handbook."

Generally, too, the various authors have emphasized the experimental finding or at least the quantitative result. Individual historical material, the clay of the analysts, receives appreciative attention only in the chapter by Dr. Phyllis Blanchard on "The Child with Difficulties of Adjustment." In fact, in her discussion the survey or statistical analyses get little mention. Dr. Blanchard's vocabulary is the traditional one of the analyst. She expresses herself as follows: "The environmental situations which according to statistical research are frequently associated with delinquency probably gain their statistical prominence because they furnish the setting for psychosexual conflicts and difficulties in making identification with parents" (862). "In our own clinic cases we frequently see how fear dreams and night terrors may be utilized by the child as a rationalization for desires to sleep with parents or to call one parent from the other into the child's bed" (866). Some materials assembled by the Adlerians and Rankians as well as by Freudians are offered for consideration—a condition which did not obtain in Anna Freud's chapter in the first edition of the "Handbook."

Were we to evaluate in detail Dr. Blanchard's presentation, we would find our discomforts stimulated mainly by certain Freudian, or at least psychoanalytic concepts or terminologies, and the use of these with a startling sweep. Since, however, the "Handbook" is essentially encyclopedic, we are not in a position to discuss carefully the contents of any of the individual chapters. In the case of the one-man expositions, comment on a few points may be in order.

Dr. Gesell in his discussion of maturation, while granting the organo-environmental continuum as a basic concept in any approach to the problems of development, still in his emphasis on "inner

determination" and "growth toward an optimum" seems to suffuse the issues with an unnecessarily mystical coloring. Perhaps he intends to be vitalistic, though it was not our impression that he does. We question whether he has worked to the full degree the concept of a tendency of significant environmental contributants toward a constancy, at least within limits sufficiently restricted as not to distort frequently the main outlines of the developmental pattern. We would not seem unappreciative of the necessity for abstraction, selection, and even emphasis upon the organism, but in a discussion of the organo-environmental problem perhaps what we need most is to have the interaction phases played upon to the full.

Dr. Blatz, in writing of the physiological appetites, disturbs us in a somewhat different way. He announces six appetites, defining them as "secondary motivating factors, influencing behavior in so far as there is the necessity for satisfying a biological need. A group of organs integrated functionally manifests a definite rhythm in continuous successive cycles, consciously experienced and represented in overt behavior by innate responses later modified by maturation, growth, and definite learning (*725–726*). Breathing is subsumed under elimination, ennui and curiosity under change. The other alleged appetites are hunger, thirst, rest, and sex. It is not clear to us just why the number of appetites should be limited to six, why, for instance, temperature, or tissue integrity needs, or many others, are left out of the picture; why the group presented are implied to have a greater inherent unity than some classification cutting across the manifold of interacting organic processes in another way; just why one should stop in the scheme of analysis with six groups or factors as they are called instead with a larger category such as self-preservation or a smaller one such as the digestion of fat. If the descriptive scheme employed were offered as dictated merely by convenience or usefulness in dealing with child management problems, we could listen sympathetically.

To summarize: *The Handbook of Child Psychology* is eclectic, catering to no one system or group; and encyclopedic, aiming to inform rather than to interest particularly, to describe rather than argue, to use categories in terms of which much of the current thinking is being done rather than to use local non-overlapping ones. It covers the field of child psychology rather thoroughly, though we still wonder at the general lack of attention to the psychological aspects of certain academic, educational, recreational, and sociological problems. The new material that has been added and the refocussing that has

occurred has changed what was an originally outstanding volume to one we can give first place among the books in our library on child psychology.

HELEN L. KOCH.

University of Chicago.

HUMPHREY, GEORGE. *The Nature of Learning in Its Relation to the Living System.* New York: Harcourt, Brace and Co., 1933. Pp. 296.

This book is grounded on the postulate, accepted by very nearly all American psychologists, that in studying psychological phenomena we cannot declare "a moratorium in the operation of elsewhere universal physico-chemical principles" (p. 5). It attempts "to show that organisms may reasonably be considered as material systems so constituted that organic events may be truly subject to physico-chemical laws, and yet, on the whole, and under the requisite conditions, actively tend in an astonishingly complex manner towards the conservation of the living system in question" (pp. 5–6). The problem of learning is restated in these terms so that learning appears as one instance of self-conserving action. Few psychologists will fail to be interested in this or in any other serious attempt to exhibit learning as a phenomenon continuous with the other phenomena of biology.

The argument involves throughout the concept of a living system and 93 pages are devoted to an examination of this concept. From an initial discussion of the meaning of system in general Humphrey passes to the specific implications of a vital system. His treatment of these concepts is clear and careful and reveals an intimate familiarity with logical theory and with contemporary biology. These pages are important entirely aside from their bearing upon the problem of learning.

Vital systems remain, throughout their continuous interchanges of matter and energy with the environment, an identity which is dynamic and progressive. They are, in this sense, comparable to the chemical systems obeying the Le Chatelier rule. They possess, also, the property of organic direction. These characteristics are offered as basic to an understanding of learning in relation to the living system.

A series of organic acts is said to exhibit learning when later terms presuppose earlier ones, "but in addition the series will generally show a general approximation to an optimal term, optimal,

that is to say, from the point of view of systemic conservation" (p. 105). Conservation of system, modification which "tends to the organism's advantage" (p. 120), is, thus, made one of the criteria of learning, and receives such an extensive use in the remainder of the discussion that it deserves comment. An adequate set of criteria of learning is admittedly difficult to formulate. The criterion just stated has at least three difficulties to meet.

(1) Advantage and conservation are very difficult to identify in a specific case. How are we to know, when an organism shows certain changes in behavior, whether the changes conserve the vital system or not? When an organism learns to get food, there is some reason to say that the system has been conserved; but when one learns the logic of Bradley, a Pindaric ode or a list of syllables, we seem to be left without means of knowing whether the system has been conserved or not. The more complex the system and the more subtle the modifications which it undergoes, the harder becomes the application of this criterion. (2) The criterion aims to make a distinction between changes in behavior which are measured by similar operations and which resemble each other in all but advantage. It may be questioned whether two classifications are necessary, especially when the division is made on a value basis. Why, for example, separate between the formation of a habit of eating spinach and one of taking opium? One is thought to conserve the organism, the other causes the organism to deteriorate, but the two seem to be alike except for value. (3) It is difficult, if not impossible, to apply this criterion without circularity. Since advantage in a given case may be impossible to identify, independently of the change in behavior, the reasoning is in constant danger of passing from learning to conservation, instead of from conservation to learning. The learning is then made the criterion of conservation without overt admission of the shift.

One of the major emphases of the book is upon the four-dimensional character of integration. Total systemic adjustments are made to total situations and in each case the space-time manifold is implied. On such a view the uniqueness of association disappears, the notion of memory, with its "illegitimate abstraction of time from events" (p. 117), is unnecessary, and successive repetitions lose their discreteness. The adjustments of the organism move toward preservation of equilibrium by virtue of synthesis and minimal reaction (see pp. 270, 273–278). There is much of value in the correction which such a view offers for the relative atomism of some treatments of association and for space-time dichotomies. Its relation to *Gestalt* theories will

at once be recognized. It runs the risk, however, of an hypostatization of total situation and of "creative integrative activity" (p. 119), and the idea of a conservative equilibrium and of minimal reaction face essentially the difficulties outlined above. The description has the advantage of being in terms of biological concepts, but the application of these concepts is in far too many cases so perfectly general as to be fruitless.

The chapters on habituation, association, the conditioned reflex, and the problem of the maze are important in their own right. They are based on experimental data from primary sources and contain, as well, critical evaluations of other points of view. As would be expected from the theoretical position taken, the conditioned reflex view of habit is discarded. The evidence offered for its rejection deserves serious consideration from advocates for the conditioned response. The reviewer doubts that the interpretative concepts employed can either extend our experimental analysis of the phenomena of learning or give us a logical organization of the experimental facts. The elaboration and defense of such concepts will, however, do much to sharpen both theoretical and experimental issues.

Humphrey does not assert that he had solved all of the problems of learning. He has written a serious, calm and scholarly statement of a biological interpretation of learning at somewhat simpler and primarily motor levels. The tenor is that of cautious theorizing and the theory developed is not offered as a general panacea for our psychological difficulties. It would be immensely important to have an acceptable biological interpretation of learning and a specific knowledge of the organic functions which underly the phenomena of performance which we measure and call learning. Whether they agree with its interpretations or not, students of learning will find *The Nature of Learning* a volume worthy of being read carefully.

JOHN A. MCGEOCH.

University of Missouri.

FINANCIAL REPORTS OF PUBLICATION ENTERPRISES OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

Psychological Review Company

Financial Report

January 1 to December 31, 1933

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JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

FINANCIAL REPORT

JANUARY 1 TO DECEMBER 31, 1933

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PSYCHOLOGICAL ABSTRACTS

FINANCIAL REPORT OF THE BUSINESS OFFICE

JANUARY 1 TO DECEMBER 31, 1933

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Psychological Abstracts

Financial Report of the Editorial Office

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Disbursements

Associate Editor's salary	$3,000.00	
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Abstractors	1,314.18	
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Editorial Office expenses	945.56	
Tax on checks	8.34	
		$7,021.79
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		$7,154.51

NECROLOGY

Joseph W. Hayes

1880–1934

Joseph W. Hayes died on March 11 at his home in New York City. Dr. Hayes received the Ph.D. degree from the University of Chicago in 1910. He was Instructor and Assistant Professor in that University prior to the World War. During the war Hayes was a major in the army. He was at first connected with the intelligence test service and later with the trade test division of the Committee on Classification of Personnel. After the war Hayes became a member of the Scott Company, Industrial Consultants. Since 1921 he has been director of research for the Crowell Publishing Company.

CORRECTION

Norman Powell is Assistant Psychologist at Sing Sing Prison and not Psychologist, as recently stated in this journal, that position being held by James Hargan.

EDITORIAL ANNOUNCEMENT: ELECTIONS TO THE BOARD OF EDITORS OF THE PSYCHOLOGICAL REVIEW PUBLICATIONS

The Council of the American Psychological Association has confirmed the election by the Board of Editors of the Psychological Review Publications of Herbert S. Langfeld, Princeton University, as Editor of the *Psychological Review* to succeed the late Professor Howard C. Warren, and the election of Joseph Peterson, George Peabody College for Teachers, as Editor of the *Psychological Monographs* to succeed Professor Langfeld.

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